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june
1976

CATA
GEO

STROPHIST
LOGY

a magazine dedicated

to the study

of discontinuities

in EARTH HISTORY

A magazine dedicated to the study
of discontinuities in Earth history

Founder: Johan B. Kloosterman, field geologist
Caixa Postal 41.003
Rio de Janeiro / B R A S I L

Price for four quarterly issues Cr\$ 100,- in Brazil, US\$ 10,-
outside Brazil. If desired add US\$ 2,50 for airmail.
Please send money-orders in favor of Catastrophist Geology
through Banco Holandes Unido S/A, Caixa Postal 1242-ZC-00,
Rio de Janeiro.

This introductory issue has been composed by the founder. In the
future, editorial responsibility will be shared; permanent col-
laborators will be organized in the following groups:

- a) an editorial board composed of people working in various
branches of geology and palaeontology;
- b) a board of advisors composed of people working in non-geo-
logical sciences: physics, mathematics, chemistry, astronomy,
biology, history, archaeology, anthropology, etc.;
- c) a group of correspondents composed of geologists, palaeonto-
logists and others, who have some special knowledge and may
occasionally be relied upon for advice or help.

The following persons have manifested their willingness to sit
on these boards or to act as correspondents:

Doeko Goosen	pedologist	Enschede/Holland
Alistair Pitty	geomorphologist	Hull/Great Britain
V. Axel Firsoff	astronomer	Glastonbury/Great Br.
Horace C. Dudley	physicist	Chicago/U.S.A.
G.W. van Oosterhout	chemist	Delft/Holland
E. Nunes Pereira	anthropologist	Rio de Janeiro/Brazil
Ewoud H. Bon	exploration geologist	Amsterdam/Holland
David J. Thomas	palaeontologist	Oswego/U.S.A.
Ron Middleton	exploration geologist	Rio de Janeiro/Brazil

The examples also illustrate that geologists are overly afraid of physicists; meanwhile they continue to adhere to a natural philosophy which physics has discarded since Einstein's youth. If there is an inconsistency between geological features and accepted physical laws, we should not wait for the physicist's approval, but should notify them that there is something missing in their system. Both geology and physics could benefit thereby.

Catastrophism admits the occurrence of discontinuities in Earth history — because we observe them now and because we are forced to infer them from the geological record. Even such Lyellian agents as the raindrop and the sandgrain often do their work in discontinuous manners: the catastrophic erosion after a lake-spill for example, or sedimentation by turbidity current. In spite of our proclaimed uniformitarianism, catastrophist hypotheses abound — the capture of the Moon, astroblemes, bursts of cosmic rays, natural nuclear reactors, the breaking up and the collision of continents. When proposed by geologists of non-catastrophist persuasion, such hypotheses are taken seriously, but when similar ideas are forwarded by less conditioned outsiders, they are regarded as evidence of lunacy simply because they violate uniformitarian dogma. Mainstream geologists often do not even try to formulate clearly their own ideas; while they are cheating, somebody relegated to the lunatic fringe may be exposing the fraud.

Catastrophes do occur. The dinosaurs did die out — whether it took a million years or a day — either through the cumulative effect of continuous causes, actualistic or not, or through a unique, sudden, terrestrial or extraterrestrial event. Should such riddles ever be solved, the solutions will come from an inspired search for clues and not through application of the methods of medieval scholastics or nineteenth-century rationalists.

"CATASTROPHIST GEOLOGY" will be devoted to the study of discontinuities in Earth history. Papers are invited on topics such as (in alphabetic order): alpine nappes, atmospheric history, the appearance of new life forms, astroblemes, astronomical influences, bone breccias, calderas, the capture of the Moon, climatic change, comet flybys, continental break-up, continental collision, earthquakes, extinction of life forms, guide fossils, historical catastrophes, the history of evolutionary thought, the history of geology, ice ages, lake spills, lunar volcanism, magnetic reversals, martian volcanoes, the Mesozoic-Neozoic boundary, meteor impacts, mudflows, natural nuclear reactions, the origin of life, orogenesis, the Permo-Trias boundary, the philosophy of geology, plate tectonics, plateau basalts, the Precambrian-Cambrian boundary, prehistoric catastrophes, pre-plate-tectonic crustal processes, regressions, sealevel changes, the survival of "living fossils", tektites, transgressions, tsunamis, turbidity currents, volcanic eruptions.

"CATASTROPHIST GEOLOGY" is intended to be of academic level, but it is a counter-culture magazine which also wants to explore possible aspects of reality that have been kept outside the walls of Academe because of their clash with the reigning preconceptions of the scientific establishment. What scraps of Earth history might be preserved in myth and legend? Has evidence for a worldwide flood been suppressed, deliberately or unconsciously? Many millions of people accept Atlantis as a historical fact: must geologists tell them that they are hopelessly wrong? Is Velikovsky a charlatan or a genius, or both? The case for Ufo's has been reopened, this time by astronomers of repute: if aliens interact with mankind, what might be their role in Earth history? If telepathy and telekinesis occur, what are the implications for evolutionary theory? Can the claims of water and ore diviners be verified through scientific methods?

Special issues will be devoted to such topics, giving space to both proponents and critics.

It is intended to start with a quarterly journal, hopefully at a relatively low price — that is, if enough subscribers turn up. Ideas and suggestions are solicited, as well as criticism. Manuscripts are invited.

The preceding text was distributed, as a circular, amongst the participants of the Charles Lyell Centenary Conference, held in September 1975 in London. Then, little by little, four thousand copies were sent to geologists, to universities and to journals. I had reasonable address lists only for Holland, France and the U.S.A., so it was not surprising that much of the response was from these countries. However, it seemed as if the journal started to have a life of its own even before the first issue had appeared: letter writers were geographically concentrated in the U.S.A., France and in Great Britain; articles came mainly from the U.S.A., Holland and from Canada.

In the following section an anthology of quotes is given. They were selected from letters received in the period October 1975 to April 1976.

comments

Derek V. Ager	Swansea, Great Britain
Claude Albritton	Dallas, U.S.A.
John M. Bell	New York
Aart Brouwer	Leiden, Holland
R.H. Brown	Berrien Springs, U.S.A.
George Cuvier	Paris
Horace C. Dudley	Chicago, U.S.A.
Farouk El-Baz	Washington D.C.
F.J. Faber	Voorst, Holland
V. Axel Firsoff	Glastonbury, Great Britain
Peter E. Gretener	Calgary, Canada
Ralph von Koenigswald	Frankfurt, West Germany
Arthur Koestler	London
Emanuel Levine	Lawrenceville, U.S.A.
Andrew J. Mantura	Beirut, Lebanon
Joseph May	Youngstown, U.S.A.
Pierre Routhier	Paris
C.L. (Pete) Sainsbury	Evergreen, U.S.A.
W.F. Tanner	Tallahassee, U.S.A.
Haroun Tazieff	Paris
René Thom	Bures-sur-Yvette, France
David J. Thomas	Oswego, U.S.A.
William Irwin Thompson	Southampton, U.S.A.
Harold Tresman	Elstree, Great Britain
Nicolas Varlamoff	White Plains, U.S.A.
Jean Vogt	Orléans, France
E. Wegmann	Neuchâtel, Switzerland

Since (your proposed journal) seems to be so much in line with my own ideas - as expressed in my book "The Nature of the Stratigraphical Record", Macmillan's 1973 - I am naturally very interested in the project. Whilst remaining, I hope, broad-minded I must express strong doubts about the desirability of including discussions of some aspects of the "lunatic fringe". Thus my book brought me much literature from the Velikovsky band, but when I asked for concrete geological evidence there was immediate and complete silence. "Neocatastrophism" is, in my view, a valid scientific hypothesis that deserves more careful attention by geologists, but we will not acquire scientific respectability if we associate ourselves with Atlantis-hunters or, for that matter, flat-earthers!

Derek V. Ager
Dept. of Geology and Oceanography
University College
Swansea, Great Britain



I think that it is very important to keep an open dialogue with aberrant minority groups. As a matter of fact I regard willingness to listen to their opinions as the very touchstone of openmindedness. The question to ask is: can we enter into a useful discussion with them, even if it were only to learn where their arguments go wrong? For Velikovsky and the Atlantologists I would answer this question with a yes, for the flat- or hollow-earthers with a no. After reading more on these subjects I could of course revise my opinion. Anyway, dedicating special issues to these subjects does not imply endorsement, so I do not really understand your cautioning. It sounds as if you were afraid of the uniformitarians - people that start from the wrong assumptions and then proceed to suppress part of the evidence - but you do keep an open dialogue with them, don't you? So why not with Velikovsky? If we close the dialogue, we are in danger of becoming dogmatic - that is what happened to the uniformitarians. And if we have to do so in order to acquire scientific respectability, I think the price is too high!

Han Kl.

I accept your point about the so-called "lunatic fringe", but I have really tried to come to terms with the Velikovskians without success. I am not competent to argue with their astronomical ideas, but they have so far failed to produce anything that fits in with my own observations in geology. I have positively begged them to produce some geological evidence but at that point the correspondence ceased. I am not "afraid" of the uniformitarians, I am just concerned that if you devote space to these matters you will not be taken seriously by the ordinary open-minded geologists (and they are, on the whole, open-minded, as I found from the response to my book). Take "Atlantis", for example. I have no doubt at all that there never was such a land-mass (at least in man's time) beyond the "Pillars of Hercules". That subject is just not worth discussing. On the other hand I think it quite possible, even probable, that the legend refers to the destruction of the Minoan civilization by volcanic processes. This is a problem for the archaeologist rather than the geologist.

Personally I would rather not be tarred with a Velikovskyan brush, at least not until they are a lot more scientific in my field than they have been to date. On these grounds I would be hesitant about publishing in such a journal. Nevertheless I wish it well and hope that my doubts are unfounded.

You may like to know that my presidential address to our Geologists' Association in March 1976, entitled "The Nature of the Fossil Record" will treat neocatastrophist themes in palaeontology. I prefer to use the term "neocatastrophism" - like "neodarwinism" - to distinguish it from the ideas of the Noachian deluge people (who are still around).

Derek Ager.

I am sorry about your 'hesitancy about publishing in such a journal'. I have read very little on Atlantis, but I can give at least one example. The discovery of subaquatic megalithic ruins off the Bahamas is given by Atlantologists as evidence for their theses. According to them, the ruins are more than ten

thousand years old; the flooding thus could be due to sealevel adjustment during melting of ice-cap ice. But if it could be shown (for instance on geological grounds) that there has been a more recent subsidence in the area, these ruins possibly would fit into Thor Heyerdahl's theory of megalithic cultural contacts between the Mediterranean area and Central America. In the meantime, we would be learning something about rates of subsidence. Not the first time that there would have been a cooperation between geologists and archaeologists, but in this case the ruins were discovered by Atlantologists.

I cannot help but find the comparison of Atlantis with Thera a little awkward, especially now there is such a good possibility to equate Atlantis with America, after the work of Heyerdahl, Cyrus Gordon, and many others. If this equation is valid, the legend may reflect a historical catastrophe other than the Thera eruption and seems certainly worth investigating. Neither should we dismiss offhand the age given by Atlantologists: see the article by Emiliani et al. in the September '75 issue of Science.

I would very much appreciate receiving a reprint of your presidential address.

Han Kl.

In the event you decide to proceed with publication of (Catastrophist Geology) I should like to purchase a subscription.

My interest in your project should not be taken as an endorsement of the views stated in the brochure.

Claude Albritton
Dept. of Geology
Southern Methodist University
Dallas, U.S.A.

Claude Albritton

Yours sounds like an exciting and necessary project. I wish you good fortune with it.

John M. Bell
Dept. of Humanities,
New York University
New York



Uniformitarian catastrophes

(for references see p.21)

A growing misunderstanding regarding uniformitarianism as a basic concept in geology seems at present to develop due to confusion of causes, processes and results. The leaflet issued to announce the publication of this magazine is a good example. It starts by trying to define uniformitarianism in terms of 'processes' operating 'at the same intensity and rate as now'. Charles Lyell, whom we still may consider as the founder of the concept of uniformitarianism, was well aware of the difference between causes, and the resulting processes and effects observable to geologists. Already the subtitle of his Principles of Geology leaves no doubt that Lyell is dealing with causes. He must have realized that the sea-level oscillations beautifully shown by the pillars of the temple of Serapis (frontispiece of volume I) could well have been experienced as a catastrophe by the medieval Romans. Nevertheless they served Lyell as an excellent illustration of uniformitarian principles (Lyell, 1830, p.449-56).

We tend to forget that the nature of the processes and the recorded results eventually produced depend on the material upon and the conditions under which the causes operate. As 'anybody can observe by placing a pan full of milk on a stove' (to quote the leaflet) the effect in the next few minutes depends on whether the milk is cold or near boiling point. There is no need to rely upon non-uniformitarian causes to explain the capture of the Moon. What matters is whether a celestial body of suitable dimensions arrives within the Earth's gravitational field. The number of such instances where natural causes lead to quite different, and even catastrophic results, can easily be multiplied. Also in the early history of the Earth the concept of uniformitarianism seems perfectly applicable, as long as we take into account that conditions then were different from what they were later, with respect to the amount of radiogenic heat, thickness of the crust, and probably the dimensions of continental plates. Such essential differences are sometimes forgotten. L.Cayeux (1941) made a similar mistake when he questioned the general validity of uniformitarianism after comparing the amount of recent carbonate sedimentation in warm,

shallow seas with the tremendous record of past periods (e.g. Lower Carboniferous, Jurassic and Cretaceous). What he neglected was that present-day climatic and morphological conditions of the Earth differ from those in some earlier periods, with essential and unavoidable effects on the general sedimentary pattern of shallow seas (Brouwer, 1962).

Catastrophic events can and do result from natural causes. From earthquakes to floods, and from volcanic eruptions to meteoritic impacts they belong to the chain of events brought about by uniformitarian causes. But perhaps geologists have underestimated their importance when deciphering the geological record. In a mere hundred thought-provoking pages Ager (1973) has recently called attention to the way in which the stratigraphic succession reflects this type of events. Such catastrophes, however, are fundamentally different from those advocated by early nineteenth century opponents of the doctrine of uniformitarianism inspired by Cuvier's 'révolutions du globe'. One wonders whether Catastrophist Geology, 'dedicated to the study of discontinuities in Earth history' tends to emphasize the consequences of catastrophism as opposed to uniformitarianism, rather than placing catastrophic events in their proper perspective, i.e. as the result of natural, uniformitarian causes. Darwin already was perfectly aware of the occurrence of hiatuses in the stratigraphic record. Perhaps he even overemphasized their importance, which did not prevent him from being among the earliest adherents of Lyell's uniformitarianism.

A. Brouwer
Geologisch Instituut
University of Leiden
Nederland



It is neither Cayeux nor the catastrophists but the uniformitarians who tend to forget the "essential differences" between present-day conditions and those in earlier periods. The (uniformitarian) misunderstanding regarding methods in geology has developed owing to a confusion of causes, processes and results, and also because of the ambiguity of the concept 'natural' (as exemplified twice in your letter). We do not deal with

causes without a cause, but each cause in its turn is the result of a combination of processes, and so on, until we reduce the content of uniformitarianism to the constancy of natural laws. And these are man-made, they change as a function of our discoveries and of our philosophical outlook. "Catastrophic events can and do result from natural causes", as you write, and maybe also from less 'natural' ones.

This journal will emphatically not be restricted to "placing catastrophic events in their proper perspective". Evidence suggesting that there is something wrong with our 'proper perspective' will be accepted also.

Han Kl.

I have a keen interest in your proposed publication, and trust that it will achieve success beyond your anticipations.

R.H. Brown
Berrien Springs, U.S.A.



C'est avec beaucoup d'intérêt que j'ai pris connaissance de votre projet. Il est sans doute nécessaire de soumettre à un nouvel examen critique les suppositions de base sur lesquelles reposent nos sciences ! Nos écoles organisées il y a un siècle et demi par Cuvier dans sa fonction de Maître de l'Université reflétaient à l'époque une réalité sociale. Grâce à ses judicieuses mesures d'alors, le développement scientifique du XIX siècle put se faire en France, avec l'éclat que l'on sait. Et pour ne prendre qu'un exemple, c'est grâce à ses "bourses d'études", que le fils d'un "grogard" de Napoléon Ier, Louis Pasteur, put devenir un des grands bienfaiteurs de l'humanité.

Mais les dirigeants postérieurs n'ont pas eu à temps assez d'avenir dans l'esprit. Puis, ils ont précipité irrationnellement le rattrapage nécessaire. Le modernisme est devenu le moteur presque exclusif du progrès, et la nouveauté un nouveau dogme, alors qu'il y a dans la Nature des éléments de fixité qui ne peuvent être négligés sans danger. L'adaptation est l'élément régulateur à réaliser, sous peine d'insuccès, soit par bouleversements inconsidérés, soit par routine sclérosante.

De nos jours, on entrevoit dans les doctrines de Cuvier des tournures de pensée qui peuvent nous aider à retrouver une approche des faits qui a été abandonnée par l'esprit trop analytique qui lui a succédé. Car il avait, lui, un esprit très intuitif, on pourrait presque parler de la "divination" des sciences nouvelles qu'il a créées: l'anatomie comparée, la paléontologie. Il dut s'effectuer dans son esprit le phénomène de "cristallisation" que Stendhal, l'hôte critique de ses réceptions réputées, devait appliquer à la formation de l'amour. Il perça un des secrets les plus importants de la Nature, celui qui allait lui donner une renommée presque légendaire. C'est de ses vues -- que tout système organique est à la fois un et divers, synthétisant toutes les activités de ses composants pour aboutir à des pouvoirs nouveaux et inattendus -- que découlaient ses classifications en zoologie, en anatomie comparée, en paléontologie. Selon les mots propres de Cuvier:

"Tout être organisé forme un ensemble, un système clos, dont les parties se correspondent naturellement et concourent à la même action définitive par une action réciproque. Aucune de ces parties ne peut changer, sans que les autres changent aussi, et par conséquent chacune d'elles prise séparément, indique et donne toutes les autres."

Aujourd'hui, on changerait quelques mots (système harmonieux pour système clos), mais pas le sens général de son affirmation. Si Linné s'était basé sur les propriétés superficielles (en classant par exemple les baleines avec les poissons), Cuvier essaie de s'en tenir aux caractéristiques anatomiques du corps entier. En anatomie, si l'on s'était occupé, depuis Aristote, du corps humain seul, Cuvier envisagea pour la première fois la totalité du règne animal. En paléontologie, sa conception est à la base de sa facilité prodigieuse à reconstituer les animaux disparus depuis des âges révolus, en partant d'un seul de leurs os.

Il voyait clairement le principe, que tel caractère organique est nécessairement suivi d'un autre qui le complète, ou le coordonne à l'ensemble des éléments formateurs d'un être - conditionnant par là, sa qualité propre d'être vivant. Ce principe lui a donné la clé du monde endormi des fossiles. Il dévoila un passé impressionnant et lointain, qui était insoupçonné, sinon même impensable jusqu'alors.

Il m'a toujours semblé important de propager ses méthodes et son système, dans l'espoir que les générations futures y retrouveront des idées utiles, et retourneront à certaines d'entre elles. Je me demande par exemple si le simple jeu d'une sélection néo-darwinienne suffit vraiment à expliquer l'évolution? Quelle est la signification de ces "fossiles vivants" énigmatiques, qui peut-être justifient en partie la loi de la permanence des espèces? À quoi tient la stabilité du *Certodus*, du *Coelacanth*, des termites, des limules, du nautilus, du varan de Komodo, et aussi de certaines espèces végétales, qui n'ont ni disparu, ni évolué? En somme, s'il y a des espèces qui évoluent lentement, il y en a dont l'évolution paraît s'accélérer pendant certaines époques, et il y en a d'autres qui s'éteignent en masse; il y en a, enfin, qui n'évoluent pas du tout. Peut-être devra-t-on en venir un jour à une synthèse qui inclura à la fois, une évolution néo-darwinienne, et des mutations brusques, suivant la conception de Cuvier.

George Cuvier
Paris



(Dr. Cuvier is not a descendant but belongs to the same family as his famous namesake. He is a medical doctor who has founded the review "Bulletin de Biologie Clinique" and has published on social hygiene, biometeorology, work safety, and on the history of science).

Geologists should never stand in awe of physicists!



Horace C. Dudley (physicist)
University of Illinois
Chicago, U.S.A.

The magazine that you are founding certainly appears to be interesting. This must be a challenge to you at this time.

At present I am extremely busy in preparing the science report on the Apollo Soyuz mission since I was Principal Investigator for "Earth Observations and Photography". However, I will be glad to write a paper on the Moon for your magazine in the future.

Farouk El-Baz
National Air and Space Museum
Smithsonian Institution
Washington

Farouk ElBaz

Graag zou ik meer inlichtingen krijgen over het nieuwe tijdschrift. Ik interesseer me bijzonder voor het catastrofisme in de geologie, dat zelfs onderwerp was van mijn oratie als hoogleraar in de geologie aan de Technische Hogeschool in Delft.

Ik hoop dat U zult slagen in Uw opzet om het tijdschrift van de grond te krijgen: ik weet hoeveel werk daaraan vast zit.

F.J.Faber
Voorst, Holland

F.J. Faber

Your magazine sounds very interesting. Lately my impression has been that much of what Thomas Kuhn has called 'normal science' has degenerated into mindless support of orthodoxy and the so-called 'consensus of opinion', which is arrived at by a process of one scientist repeating what another has said in a kind of mirror-gallery regression for the fear of falling out with his (or her) colleagues. In the end nobody seems to know how this 'consensus' has originated, but anything that is out of step is ruthlessly suppressed. The comments of some editors' referees are unbelievably idiotic.

Inasmuch I am certainly on your side, though basically I am a 'uniformitarian': I don't like the 'terrorist geology of impact', which is not the same thing as saying that no impacts have ever occurred. There is nothing impossible even about a catastrophic one, and gradual processes may build up to a crisis situation in which cumulative stresses are violently released. Some kind of balance must be preserved here. On the other hand, it is definitely a mistake to imagine that conditions must have always been the same as now. There

are cyclic and secular changes creating epochs of great disparity.

Best wishes for you and your venture.

V. Axel Firsoff
Glastonbury, Great Britain

axel firsoff

I share your distrust of the more spectacular catastrophist hypotheses. But I wonder to what extent we have to blame our uniformitarian training for such preferences?

Han Kl.

Best of luck !

Peter E. Gretener
Dept. of Geology,
The University
Calgary, Canada

Peter Gretener

Best wishes !

Ralph H. von Koenigswald
Senckenberg Museum
Frankfurt, West Germany

R. von Koenigswald.

I am in full sympathy with your endeavour, and you can count on me as a faithful subscriber to the planned magazine.

What you say in your pamphlet strikes me as making eminently good sense and fitting well into the anti-reductionist, pro-Systems Theory approach. I shall be looking forward to seeing it expounded in your magazine.

A. Koestler

Arthur Koestler
London

I received your announcement concerning a new journal, and what you say seems exciting. What we need are scholars with open minds, who can dispassionately and objectively explore every possibility in getting at the truth. My very best wishes to you for its success.

Emanuel Levine
Dept. of History and Archaeology,
Rider College
Lawrenceville, U.S.A.



I congratulate you on your initiative and degree of foresight, as uniformitarianism is indeed becoming too tyrannic in its hold on the Earth sciences - and this despite its flagrant imperfections. This doctrine could be summed up in the following words: "since a car is built to run, it has no right (and no chance) of an accident", which is an absurdity per se.

You are providing Geonomy with a much-needed free forum, which has so far been strangely lacking.

I wish your journal all the success it deserves.

Andrew Mantura
Beirut, Lebanon



Those of us who are personal friends of Velikovsky resent his being referred to as a charlatan because we know him to be a sincere and dedicated seeker of the truth who is being repaid in such coin primarily because he is a catastrophist. There really isn't any reason for mentioning the word except as an example of the kind of extreme emotionalism and unreason which automatically equates catastrophist ideas with fraud.

You can expect the same treatment.

Good luck on your venture.

Joseph May
Dept. of History
Youngstown State University
Youngstown, U.S.A.



Somebody -I forgot who- called Velikovsky "our most erudite charlatan". I referred to that in order to make clear that in a special Velikovsky issue I want both detractors and defenders of his theories to present their arguments.

Han Kl.

The characterization "one of our most erudite charlatans" is Harlow Shapley's (see Pensée I, p.36).

Joseph May

Votre projet me paraît justifié, provoquant et amusant. Je lui souhaite bonne chance et il n'est pas impossible que je vous donne, un de ces jours, un papier sur la métallogénie régionale.

Quelques remarques :

-Votre commentaire sur l'"actualisme" (en français et seulement en français) n'est peut-être pas tout à fait rigoureux. Il repose en partie sur le contresens qu'avaient commis tant de Français à propos des "causes réelles" ("actual"). Lucien Cayeux a même écrit tout un petit ouvrage en partie fondé sur ce contresens !

Seul Pierre Pruvost (1956) a exprimé correctement en français le principe de Hutton-Lyell, mais en l'élargissant. Je l'ai indiqué dans mon petit "Essai critique sur les méthodes de la Géologie" (Masson, 1969) où je n'ai d'ailleurs pas été assez sévère à l'égard du principe.

-Au fond aucun géologue créatif ne s'est jamais senti enchaîné par l'"uniformitarisme". Il est donc à craindre que beaucoup de géoscientistes pensent que vous voulez mener un combat inutile. Ce n'est pas mon avis, si j'en juge par le programme que vous tracez, qui permettrait au moins de revenir à des tableaux généraux et à ce que j'appelle des "synthèses étagées"

J'espère que vous réussirez dans votre entreprise.

Pierre Routhier
Université Pierre et Marie Curie
Paris



Cependant, il faut se réaliser que les 'synthèses étagées' d'un catastrophiste seront très différentes de celles d'un uniformitariste!

Han Kl.

Please accept my congratulations on your thinking, as well as on your plans to start a new journal on catastrophism.

Too many good ideas in geology are destroyed by opposition of hide-bound reviewers, when papers are submitted that propose new or heretical hypotheses that disagree with the "establishment" - which surely exists in geology as well as in society.

I think that your journal could well be expanded to accept papers from technically competent scientists who have new ideas in any field of geology, the only requirement being that the standard editorial board from some "establishment" journal has rejected it!

C.L. (Pete) Sainsbury
Air Samplex Corporation
Evergreen, U.S.A.

Pete Sainsbury

I may wish to add your journal to our library subscription list. This will probably depend on the extent to which the journal is truly geological, in contrast with subjects which are peripheral to, but not actually part of, geology.

W.F. Tanner
Dept. of Geology,
The Florida State University
Tallahassee, U.S.A.

W.F. Tanner

Sans aller jusqu'à la télépathie ou aux Martiens dans des soucoupes volantes, je suis - vieux Wegenerien - bien de l'avis que vous exposez dans votre petit tract. Si j'ai un jour quelque chose de plus ou moins valable à dire, je vous écrirai un article.

Haroun Tazieff
Paris

Haroun Tazieff

Personnellement, je crois que les phénomènes télépathiques se rangent parmi les expériences humaines les plus authentiques. Mais cette conviction n'est pas indispensable pour pouvoir accepter la théorie des catastrophes - ou pour écrire dans ce journal!

Han Kl.

Je crois que c'est une très bonne idée de relancer l'étude des catastrophes en géologie, et ceci non seulement à cause des tremblements de terre, mais aussi parce que la tectonique des plaques me semble une très belle illustration des idées générales de ma propre théorie des catastrophes.

Je me propose de suivre votre périodique avec beaucoup de sympathie.

René Thom
Institut des Hautes
Études Scientifiques
Bures-sur-Yvette, France

RJH

I am enthused about the notion of initiating a journal of this genre. Its flavor, "academic-counter culture", definitely is needed on library shelves today, and the topics indicated for exposition in the journal are "great".

I know of no other vehicle devoted to the lines of thinking you mentioned. Your proposed journal will fill a vacuum in the overlooked, ignored and maligned realm of catastrophist geology.

David J. Thomas
Dept. of Earth Sciences,
State University of New York
Oswego, U.S.A.

David J. Thomas

Every good wish !

W. I. Thompson

William Irwin Thompson
The Lindisfarne Association
Southampton, U.S.A.

The contents of your leaflet were of great interest to me and to our society, in that we seem to have very similar aims.

This Society, founded just over a year ago, was formed by a group of laymen and academics who felt great dissatisfaction with the establishment approach to many scientific subjects. The founding group were put in touch with each other by the editor of the "late" *Pensée*, published in the USA, and our common interest was to promote a rational and quantitative examination of the theories of Dr Immanuel Velikovsky. We now have a membership of close on a hundred, and a mailing list of three hundred. Almost the entire number reside in Great Britain. In addition we have a potential mailing list of almost one thousand in Britain and in other parts of the world. We are now in the process of expanding gradually (uniformitarianism even applies to Societies?) as funds become available. As a Society we produce a journal containing original contributions, including some by contributors to *Pensée*, hold regular meetings, and plan to organise lectures on all aspects of Velikovsky's theories. As a major aim we naturally desire contacts with organisations throughout the world who have aims similar to ours.

But, let me emphasise, we are not a "Velikovsky Fan Club". However, we do believe that Velikovsky deserves a careful hearing; and we are aware that, despite the recent series of symposia in North America, there has not yet been any serious, unemotive analysis of his proposals.

There is a clear need for publications which will bring pressure to bear on those academics who have stifled rational debate on this and other important subjects. As yet, there seems to be a shortage of "hard science" contributions to such publications, the current emphasis being on myths and ancient history. It would seem that your "Catastrophist Geology" will balance this shortage.

Harold Tresman
Society for

Interdisciplinary Research
18 Fir Tree Court, Allum Lane
Elstree Herts., Great Britain



I think it is very difficult indeed to assess honestly Velikovsky's theories at this moment. We have been conditioned so long to uniformitarian thinking that we can only guess what will happen when we drop the assumptions. Possibly catastrophes did happen around 1500 and 700 B.C. But even so, it would seem that Velikovsky is explaining too much, with his electromagnetic solar system and his near-collisions. In fact, he explains everything, just as our conventional science pretends to explain nearly everything: we hear for instance about telepathy that it is "something we do not completely understand yet". To me, it shows that all conventional assumptions are wrong, as did the existence of fossils in the Middle Ages; and Velikovsky seems to me rather conventional in this respect - a son of the nineteenth century. He tries to give an all-embracing physical description of the Universe; apart from that, I think, we may believe or not in a divine or spiritual realm. In other words, I suspect Velikovsky's theories provide us with another kind of reductionism. Maybe I am wrong here, and I hope you will correct me.

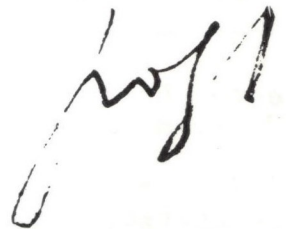
Han Kl.

Votre dépliant me parvient aujourd'hui. J'en suis d'autant plus heureux que j'ai eu l'occasion de travailler au Brésil à plusieurs reprises. En Rondonia, j'ai tiré grand profit de vos travaux personnels.

Depuis mes recherches dans l'ouest africain, je n'ai cessé de m'intéresser aux discontinuités (terme que je préfère à "catastrophes") à propos de divers sujets, à divers échelles, dans plusieurs parties du monde.

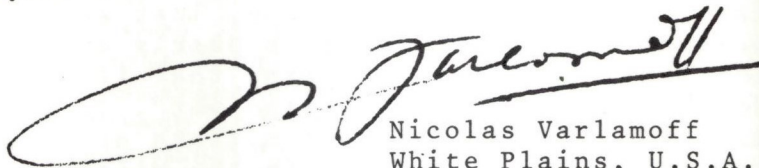
Il est vrai que la plupart de mes travaux restent confinés dans une foule de rapports, faute de temps pour en tirer des publications. Pour la même raison, j'hésite à vous promettre aussitôt un article. Je me permettrai de vous écrire à nouveau.

Jean Vogt
Bureau de Recherches
Géologiques et Minières
Orléans, France



Je me rejouis de voir sortir un journal de ce type. Je lui souhaite un réel succès et je serai certainement parmi les premiers abonnés !

Sans doute il y a beaucoup d'idées qui ont besoin d'une nouvelle appréciation critique, même la datation radiométrique ainsi que la panacée actuelle, la tectonique des plaques !



Nicolas Varlamoff
White Plains, U.S.A.

BIG-JAW VARLAMOFF IS DEAD

Born a Cossack in 1910, Nicolas Varlamoff used to tell that as a boy during a local war he nearly witnessed his grandfather's execution by hanging. Freed moments before the execution, it was his grandfather instead who hung the executioners.

Varlamoff's family was among those who resisted the Red Army. In 1922, they sailed from the Crimea to Turkey, whence they migrated to Belgium. At high-school young Varlamoff successfully struggled with a bout of tuberculosis, during which he acquired a size which became an integral part of his character and of the legends spun around it. He worked to finance his studies at Liège University, where Fourmarier was teaching then. Among other jobs, he was tile-layer on the former Guillemins railroad station.

In 1938 he started his work in the Congo, his third home-country, where his labourers nick-named him Mashavu - Big Jaw - because of his forceful construction and of the tall stories he used to tell. He was the type of man that has a better rapport with his employees than with his employers. With all his weight - almost 110 kg - he was a field geologist to be reckoned with, who surpassed most of us when it came to forest-walking or to descending in a 10- or 20-meter prospecting pit on a vine-ladder. He made pioneer studies of the tin deposits of the Zaire Republic, of the genesis of tin minerals, and of pegmatite classification, most of which were published in bulletins of the Geological Survey of Belgium.

In 1961 he changed country once more, when he became technical advisor to the U.N. Development Programme, based in New York.

On the 10th of April 1976, he suddenly died of a heart attack, only a few months after his retirement from the U.N. He was about to start a new life in Brazil, as professor of economic geology at the University of Brasilia.

Je vous présente mes félicitations d'avoir eu cette bonne idée de créer un centre nécessaire pour de nombreuses tendances.

E. Wegmann
Neuchâtel, Suisse



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the articles

SCIENTIFIC CENSORSHIP AND THOUGHT CONTROL by Horace C.Dudley corresponds to chapter four of his book "Morality of Nuclear planning - ???", published this year by Kronos Press, New Jersey, U.S.A. Copyright H.C.Dudley.

WHIMSICAL ASPECTS OF SCIENTIFIC THEORY by Norman Macbeth was written especially for this introductory issue of Catastrophist Geology.

ACTUALISM IN GEOGRAPHY AND IN GEOLOGY by W.J.Jong was originally published in 1966 : Tijdschrift van het Koninklijk Nederlands Aardrijkskundig Genootschap, 83(3) : 238-248. Copyright K.N.A.G.

GEOPHYSICAL TIME SERIES AND CATASTROPHISM by Vít Klemeš is a slightly reworked version of a lecture held in June 1974 at the symposium "Velikovsky and the Recent History of the Solar System", McMaster University, Ontario, Canada.

SCIENTIFIC CENSORSHIP AND THOUGHT CONTROL

HORACE C. DUDLEY

University of Illinois Medical Center
Chicago, U.S.A.

The Question :

One is troubled to know whether peer review stacks the deck against younger and nonestablishment scientists whose credentials are no match for those of more imposing competitors. If the system shuts out the bright beginners and independents in order to ration scarce support only to scientists whose reputations are already made, we will come to regret it.

Wm. D. Carey, Publisher
Science 189, 331, 1975

The Answer :

Do you think the present peer review system...denies funding to those individuals whose ideas or proposals in any way challenge the reviewers?

YES 87%

Are the technical journals...largely controlled by hierarchical groups that reject papers that do not fit into the paradigm mold of current theory?

YES 74%

Survey by Industrial Research
Oct. 1974, p. 97. (3,417 respondents)

Those who have progressed through any of the high school science courses have been exposed to a concept commonly called THE SCIENTIFIC METHOD. This is taught as somewhat of a creed that professional scientists live by, with near religious fervor. In essence, it means that the scientist must be a doubter, ever questioning, ever reasoning. The scientist must be guided by the evidence deduced by careful experimentation and observation. The following may be said to be the creed of one of the world's most gifted experimenters who discovered the basic principles on which today's electrical world operates:

The scientist should be a man willing to listen to every suggestion, but determined to judge for himself. He should not be biased by appearances; have no favorite hypothesis; be of no school; in doctrine have no master. He should not be a respecter of persons but of things. Truth should be his primary object. If these qualities be added to industry, he may indeed hope to walk within the veil of the temple of nature.

Michael Faraday
(1791-1867)

But those who have grubbed out their livelihood at the blackboard, the laboratory bench or at the designer's table know full well that the ideal is far more sinned against than honored.

About 15 years ago there appeared a timely article entitled, "Resistance of Scientists to Scientific Discoveries" (Barber, 1) which sketched the long history of senior scientists' inhumanity to other scientists with new ideas that differed significantly from their own cherished views, while of course being buttressed by the established climate of opinion of other senior scientists.

A study of the History of Science, any science, will show how scientists are quite human, with all the vanities of other mortals, even though to themselves, their self-image remains untarnished. In a most perceptive article entitled "Should the History of Science be Rated X" (Brush, 2), the following statement is made: "I will examine arguments that young and impressionable students should be shielded, for... these writings do violence to the professional ideal and public image of scientists as rational, open-minded investigators, proceeding methodically... seeking objectively for the truth, let the chips fall where they may."

Now turning to the History of Science, 1975, we find that there are hearings being held in the halls of Congress to examine the "Peer Review" systems which have evolved ostensibly as the means of equitably distributing the \$15 + billion dollar annual Federal Research and Development budget. There is evidence of cronyism; of faking the recommendations of referees; of all sorts of log-rolling quite as astute as in the everyday power politics at city, state and national levels (see Science 8 August 1975, p. 435) (U.S. congress Bill. H. R. 9892. Introduced 29 Sept. 1975).

And the scientific community is aghast at the suggestions of some writers that professional scientists would stoop to such practices. As for myself, having been on the science scene for nearly half a century, such revelations are not surprising, they are about 20 years overdue.

In 1959 I wrote the essentials of the following (3); it was again included in a piece published in *Science* in 1964 (4):

It is evident that Putnam and others are surprised at the "McCarthyism" that is utilized to enhance the views of F. Boas and downgrade the findings of W. C. George. But this approach has been used for centuries, and still remains, in this "enlightened" 20th century, an effective method of promoting a group's ideas, concepts, and scientific interpretations. Galileo was muzzled by cleric and scholar alike; Young's wave theory of light was suppressed for a century by Newton's idolizers...

Scientific research has become a competitive, multibillion dollar business. With bigness came administration separate from the laboratory, interested primarily in a smooth-running organization, not its principal products; data and new ideas. With affluence came the accountant, to see that monies were properly apportioned, spent, and recorded. We have thus inadvertently evolved a very efficient system of control, of checks and rechecks, of censorship by selection, which eliminates the bizarre, the unusual, the unacceptable: selection of graduate candidates and their thesis subjects; selection for membership in scientific societies; selection of papers for publication in scientific journals; selection of research projects to receive financial support.

Early in 1974 there was a flurry of excitement, generated by what was essentially a rerun of a 1950-55 scenario. This was the opening of the A.A.A.S., the world's biggest scientific society, to a discussion of the concepts of one Immanuel Velikovsky. Dr. Velikovsky had written two books some 20 years earlier that analyzed the evidence of ancient history in such a way as to question the conclusions of the professional astronomers. And this in the last analysis is the cardinal sin of Dr. Velikovsky. The scientific community responding made every effort to have these books, these ideas, banned. And this is the ultimate in foolishness that has now come back to smear so many self-images of scientific rectitude. In 1974 the same drama was played out in San Francisco, different actors of course, and different scenery. The results? The self-same foolishness, only this time ridicule was added, a two-edged sword that was wielded by the "Establishment."

My own interest in what has become known as the "Velikovsky Affair" is in the quite overt application of scientific censorship and thought-control, which is usually accomplished much more circumspectly. The following is from an editorial which was published following the A.A.A.S. meeting:

"The quantity and tone of the polarized comments, Re: The "Velikovsky Affair," which have appeared recently in a wide spectrum of publications, are symptomatic of a serious sickness that afflicts the super-organized, competitive, multi-billion dollar *business* called Science. This malaise is far more generalized than is indicated by the pros and cons on the validity of Velikovsky's postulates or his methods of presentation.

The idealized image of the scientist as an open-minded skeptic, guided by the scientific method, is convincingly sketched in high school and freshman science classes. The brutal realities are another matter — see Brush, (2). Is it any wonder that there is an anti-science backlash among so many knowledgeable people, or why so many of our brightest idealistic youngsters seek careers outside the basic sciences? The self-image so dear to the hearts of many scientists is no longer acceptable to non-scientists nor to those who control federal research funds. Altruistic pleas in the name of "Science for Science's sake" are about as effective as a buggy whip on a modern Mustang.

Scientific censorship and thought control, draped in the holy robes of the "peer review system," are being used to insure that "Them that has gits" the funding considered so necessary for the on-going research teams. Also the NIH syndrome (Not Invented Here) plays such an important part in the evaluation of research proposals by unbiased (?) peers (and competitors). One would hardly pick Ford and Chrysler to pass on General Motors' next year's designs for the rotary engine.

Just how much chance has a young unknown with a bright new idea that cuts across the cherished paradigms of the proposal evaluators? (See Jueneman and Roy, Chem. & Eng. News, Nov. 19, 1973, p. 3 and Dec. 4, 1973, p. 4).

Since the way to promotion and pay is via scientific publication, the present selection system of determining who shall publish, is the second obstacle course that the new idea, new data, must run. The kiss of death of the unbiased, fearless (and anonymous!) referee is well known to those who try to publish outside readily acceptable norms. (See Jones, New Scientist, March 21, 1974, p. 758).

And what do some 3,000 industrial scientists and engineers think of this system? Only 69% believe that the progress of science is being retarded (see June 1973, p. 77). One can only wonder what portion of the U.S. total annual research budget of 3×10^{10} dollars goes down the drain as the result of the several well-organized, smooth, censorship-by-selection procedures, which serve to snuff out new ideas and to maintain the power and prestige of those who are currently at the top of the pecking order. (Rather like some other sectors of public life that we have heard so much about lately, Yes?)

The current "Information Explosion" is another scientific revolution, which has already shown that many of our hallowed paradigms rest on sand. What is sorely needed just now is less pride in knowledge held, and more of that exciting urge for new fields to conquer, to see what is on the other side of the next hill, to recapture the free-wheeling motivation of the young scientists of 70 years ago, during *that* scientific revolution.

—H. C. Dudley
Industrial Research
 July, 1974, p.7

It is not necessary to go back 20 years to find instances of the enforcement of thought control. Fortunately, the Editors of one of our leading journals had the intestinal fortitude to stand up and be counted in an effort to see that justice be done in the case of a Nobel Prize winner who had the audacity to propose a new approach to the understanding of cancer.

Albert Szent-Gyorgyi received the Nobel Prize for Physiology and Medicine in 1937 for research on Vitamin C and fumaric acid. He has recently been endeavoring to secure funds and laboratory facilities for studies on the causes and treatment of cancer.

The following is a most illuminating statement, outlining the difficulties of this grand old man of biochemistry:

"One of the more controversial research reports C & EN has carried in recent years appears on page 16. Controversy surrounds not only the merits of Dr. Albert Szent-Gyorgyi's cancer theory, but whether it should be written about at all.

In connection with the story, C & EN consulted more than a dozen scientists, plus organizations in the cancer field. A few scientists believe Szent-Gyorgyi's ideas to be "promising and worth attention," if perhaps "way out of the mainstream." On the other hand, most consulted reject the theory, one reason being that the Nobel Laureate recently has been "off the track" scientifically.

Several scientists, however, go farther. One cancer organization executive will only comment "off the record," and says it would be best to give no publicity. And a noted scientist advises, "the less said the better" by "responsible" publications.

There seem to be four reasons for such advice: belief that the theory is so baseless and speculative it does not warrant serious consideration; concern that public criticism of the theory only would hurt a "grand old man" who has contributed so much to biochemistry; dislike for publicity and fund-raising methods used by the National Foundation for Cancer Research (a private group set up solely to support Szent-Gyorgyi's research after he had difficulty finding funding); and fear that publicity might confuse and mislead the public on a subject so sensitive as cancer.

The affair raises a dilemma for science journalists. For example notes a prominent newspaper science writer who has not reported the theory: "By the very fact you cover it (even if critically), it gives display and more importance to it."

C & EN finds compelling reasons to report on the theory that more than answer the objections above. First, although a minority, several respected scientists believe the theory deserves further attention. Szent-Gyorgyi himself expects initial criticism and rejection, even making a virtue of them. And whatever the criticisms of his recent track record or of the foundation's activities, the theory should be judged on its own merits.

Moreover, the theory has not "gone away by itself." It has been reported by, among others, the National Observer, Science News, a Paul Harvey radio network newscast, and the United Press International news-wire, and even reached media in Israel and Hungary. But coverage has been uncritical, with no reactions by other scientists, and based mainly on a somewhat misleading press release from the foundation. Those advising against reportage by responsible science writers seem a bit ostrichlike.

It is time all sides of the matter be put "on the record." The way of science is to argue an issue openly, letting scientists judge for themselves. We don't see how, as a responsible science publication, we can not cover it.

*Richard J. Seltzer
Chemical and Engineering News
28 July 1975, p. 2.*

My reaction to the above was that of disgust at the extent to which those at the top echelons of science would stoop, piously rationalizing in order that their personal empires might not be menaced in the slightest.

There is a serious sickness which afflicts U.S. science. The editorial above makes the diagnosis and pin-points the focus of infection. Censorship! Thought Control!

Having now been on the science scene for nearly a half-century, observing the extension of this disease from a mild fever to the present fulminating cancerous growth, I published in 1964 the quotation which appears above. We have now progressed (?) to the point where vaccination with a Nobel Prize offers little protection. If your hypotheses, experimental design and/or data do not fit neatly into the mainstream efforts of those who hold the purse strings or who judiciously assign research priorities, you must be off base, perhaps as reported for Prof. Szent-Gyorgyi "off the track scientifically." Review again Mr. Seltzer's editorial and you find examples of a broad spectrum of thought control. There is the "prominent science writer" who delegates to himself the prerogative of deciding what is valid news, who and what are worthy enough for ME to read about. If I should be in a position to tell HIM

what HE should publish I'm sure that I'd be hauled into the Federal courts by next week.

All this may well provide the material for a dozen future Ph.D. theses on the History of Science, 1975 or the Psychology of Rationalization in Scientific Censorship. Such sheer egotism! Such sheer disregard for the freedoms on which this nation was founded. Such sheer disregard for the very essence of all we were taught so many years ago as the SCIENTIFIC METHOD. Such desire for unbridled power to control the thoughts, actions and destinies of other human beings. HEIL! CENSOR!

- During 1975 there were Congressional hearings on the various abuses of what euphorically are called "Peer Review Systems." There has been precious little reported on this in the general media or by scientific writers. Why? The general reaction of much of the higher echelons of the U.S. scientific community is to brush all this off as a passing minor squall that will blow itself out in a few months. Then back to business as usual.

Perhaps it would be smarter to take a careful look at the results of a survey taken among working scientists and engineers.

Do you think the present peer review system...denies funding to those individuals whose ideas or proposals in any way challenge the reviewers? YES 87%

Are the technical journals... largely controlled by hierarchal groups that reject papers that do not fit into the paradigm mold of current theory? YES 74%

(3417 Respondents)

Somewhere in all this there is a message for those who administer the total annual U.S. Research and Development budgets of \$35 billion, which is our insurance of survival in the uncertain future. And certainly there needs to be some policing of those who with essentially no accountability, control such a respectable portion of the GNP.

There is a considerable credibility gap showing up as the research review processes, which have grown up like Topsy, begin to show a seamy side.

It is time that the scientific community face up to the unpleasant fact that we now stand before Legislators and the general public, not clothed in the fine raiment of our self-image but much more like the King, when the little boy says, "But Daddy, the King has no clothes on at all."

Three cheers for the editors of *Chemical and Engineering News* and *Industrial Research* for following the pathway of journalistic morality, and for seeing that far too much of what passes for scientific reporting has been carefully washed, screened and filtered to support established reputations, pet projects and ever bigger slices of the shrinking research pie.

Having discussed the problems of scientific censorship in the fields of Astronomy and Medical Biochemistry, let us now turn to that area which is our major interest in this volume, *namely nuclear science and nuclear safety.

Elsewhere in later chapters there are developed concepts which require a marked revision of the now accepted ways of going about the business of nuclear physics. Consistently, the source of nuclear energy is not considered to be $E = MC^2$, but is instead a return to a time, pre-1920, when the following paragraph was published in a standard textbook of physics:

To account for the transmission of waves through space containing no ordinary matter it seems necessary to assume the existence of a universal medium filling all space and even interpenetrating matter itself, as shown

by the existence of transparent substances. That this medium can react on matter is shown by the fact that radiant energy is transmitted from ether to matter in the case of absorption, and from matter to ether in the case of emission of radiation by material sources.

In recent years doubt as to the necessity for assuming the existence of an ether has been expressed by some who believe that it is sufficient to attribute the power of transmitting radiation to space itself. It may be doubted whether this is more than a dispute about terms. We cannot discuss the question here, but pending the settlement of the controversy it seems wise to continue the use of the word ether as at least denoting the power of space, vacant or occupied by matter, to transmit radiation.

— A.W. Duff, editor TEXT BOOK OF PHYSICS
P. Blakiston's Son and Co. 1912, p. 565.
Chapter on LIGHT, by E. Percival Lewis.

As will be made clear in ensuing chapters, the old concept of an "ether" has given way to the "neutrino sea" acting as an energy-rich subquantum medium. And it is the potential energy content of this particulate medium which is postulated to be rendered observable in nuclear events. Such is the major point of schism between what is developed herein and what has appeared in the textbooks of physics for almost 40 years.

The above may well be anathema to those who are indoctrinated in, or perhaps better yet, converted to a quasi-religious faith in a set of assumptions and postulates laid down pre-1930. Physics has not for many years been a physical science, a study of interactions of matter and/or energy. In place of a body of knowledge based on experimentation, there has evolved a series of mathematical-philosophical games which have only a nodding acquaintance with the real world in which we live, and in which nuclear power reactors operate and nuclear explosions take place. Examples of these games are given in Chapter One.

Are there instances of censorship in the field of nuclear physics? Most certainly! One of the most clearcut examples of such is the rejection of the papers by J. L. Anderson who showed that the modes of beta decay of three radioactive isotopes could be altered by rather simple means. These papers may well be classics in the future, for their long-range theoretical significance seems at this time to be assured. These papers were rejected by editors of the American Physical Society, and were later published by a journal of the American Chemical Society.

Five papers of my own were rejected by the A.P.S. editors, with three later published in the journals of the Italian Physics Society, and others in U.S. publications.

Perhaps one of the clearest expositions of the closed, restricted state of U.S. physics is given in the statement of policy of the editors of the official organ of the American Association of Physics Teachers:

*Papers announcing new results of original research are generally more appropriately published in one of the research journals...Papers presenting original research that clarifies past misunderstandings or allows a more encompassing view of a subject are certainly acceptable. Controversial or isolated new results yet to be judged in the research literature are not acceptable. Included in the latter category are papers purporting to discredit bodies of physical theory (such as special relativity, quantum mechanics, and thermodynamics) which are a part of the generally accepted physics curricula. Although these theories are not necessarily complete and immutable, a serious criticism of them is more properly presented to the appropriate specialist audience.**

American Journal of Physics
Volume 43, 1975, p. 1.

*May I ask where?

In the above quotation the words *PURPORTING* and *GENERALLY ACCEPTED PHYSICS CURRICULA* have been italicized for these are the key to the current restrictive practices and policies of this and other "respectable" journals of the U.S. physics community. For in specifying Relativity, Quantum mechanics and Thermodynamics as areas in which considerable numbers of unacceptable manuscripts are being received, it is made clear that a goodly number of physicists are dissatisfied with the methods used to develop the concepts which are so very basic to the presently accepted methods of explaining observable events.

But the criticism of these concepts or of the conclusions which are drawn therefrom will not be tolerated. This is confirmed when one examines the policies of all of the APS journals as stated by the editor-in-chief, 1973.

The methods by which reviewers, funding agencies, manuscript referees, research proposal reviewers are able to stifle the creativity is quite well outlined in the article by R. J. Seltzer, above. And when these practices delay the open-minded evaluation of Velikovsky's interpretation of events millennia past, no damage is done except to the vanity of his followers. When the new approach to the causes and treatment of cancer is delayed, as in the case of Szent-Gyorgyi, there is no long-range harm done; except of course in the case of future cancer victims, the new knowledge coming too late.

But by what school of philosophy, of ethics, is it possible to justify delaying the development of new knowledge and understanding of nuclear processes? In effectively preventing the examination of alternative hypotheses; insisting that only that which is "a part of the generally accepted physics curricula" shall be heard, insisting that our students shall not be contaminated by untested concepts, how is it possible to progress beyond our present state of half knowledge?

Theoreticians and most nuclear physicists see as actual events, the mathematical calculations which are only marks on pieces of paper or computer read-outs. These are more remote from reality than were the cloistered monks of yester-year. For while they calculate, the rest of humanity must live in a world in which MIRVA rockets can spew out H-bombs like so much confetti; when fission reactors multiply like rabbits.

Those who calculate, prognosticating as to the probabilities of nuclear accidents, have you ever personally observed at close range a nuclear explosion? I have, twice. Have you ever received an overdose of nuclear radiation? I have, several times. So it would appear that I have the vested right to challenge your calculations, for in them I see fallacious reasoning which carries a menace for my children, and their children.

If the reader has come thus far in this volume, you then realize that I have invited a deluge of ridicule from certain segments of the scientific community, for this is becoming more and more the weapon of those who must hold unsullied, at all cost, the status quo of concepts learned long ago.

But the author has seen much water go under the keel, and has ridden out many a blow. But never at anchor. There's the gauntlet, gentlemen.

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WHIMSICAL ASPECTS OF SCIENTIFIC THEORY

NORMAN MACBETH

Springfield, Vermont

U.S.A.

My training was in law rather than biology, but around 1960 I began to study Darwinian evolution theory and since that time I have read about three hundred pounds of biological literature. The bulk of this concerned plants and animals, but a good deal of it dealt with theory, with the proper formulation of and attitude toward theory. I found some strange and puzzling things that one would never have expected among sober scientists. Here are some of the most striking.

1. *Best-in-field is good enough.* Darwinism has had to compete with various rival theories, the most famous of which are vitalism, creationism, Lamarckism, and the hopeful-monster suggestion of Goldschmidt. The Darwinians have shown that none of these theories are any good. A trained evolutionist can shoot them down with ease. Thus the Darwinians are able to say that Darwin made a better try than anyone else, and they find real comfort in this.

Does this mean that Darwinism is correct? No. It means only that it is better than the other theories. But when they are no good, this is faint praise. There is no glory in outrunning a cripple.

Sir Karl Popper once deemed it prudent to disarm potential critics by making a statement about Darwinism. He wrote: "I see in modern Darwinism the most successful explanation of the relevant facts." I wrote to Sir Karl, pointing out that he was really saying very little when there were no other successful explanations. I asked whether any evolutionist had noticed how meager his commitment was. He replied that nobody had remarked on the point. In the biological world it seems that best-in-field is good enough.

2. *Look for a weak rival.* If best-in-field is good enough, a scientist will always be comfortable if his theory has only weak rivals. As Gertrude Himmelfarb remarked, Darwin's best asset was the bankruptcy of his opponents. Herbert Spencer (inventor of the phrase "survival of the fittest") was convinced that the only rival to natural selection was special creation and that this was "intrinsically incredible". Therefore he was not disturbed when T. H. Huxley pointed to glaring flaws in Spencer's own arguments. He might be weak, but his rival was weaker.

This tactic is not a monopoly of evolutionists or biologists. Harlow Shapley, the astronomer, asserted that cosmic evolution was a good hypothesis because "no other supposition is at this moment tenable." C. W. Ceram, in contending that North America was peopled by way of the Bering Straits, made the following illuminating admission: "But the present certainty rests on a rather indirect type of proof: the fact that all other theories have been eliminated as wrong or ... as simply nonsensical." Indirect indeed! Is it proof at all?

3. *Show me something better.* Courtroom experience during my career at the bar taught me to attach great weight to something that may seem trivial to persons not skilled in argumentation — the burden of proof. The proponents of a theory, in science or elsewhere, are obligated to support every link in the chain of reasoning, whereas a critic or skeptic may peck at any aspect of the theory, testing it for flaws. He is not obligated to set up any theory of his own or to offer any alternative explanations. He can be purely negative if he so desires.

The above statements seem simple and obvious, but they do not prevail in the Darwinian world. The convention there is that one should not criticize unless he can offer something better than what he attacks. I am not the only one to notice this: G.A. Kerkut remarks with irritation that, when he questions certain points, the evolutionists say in an accusing tone of voice: "What better theory have you got to offer?"

Walter J. Bock says: "Those workers (e.g. Russell, 1962) who point out the weaknesses in the synthetic theory concentrate upon this aspect, but they have been unsuccessful in remedying the fault." Thus he implies a duty to remedy a weakness before pointing it out, although in most cases this would shut off all discussion. Eldredge and Gould are more explicit: "If we doubt phyletic gradualism, we should not seek to 'disprove' it 'in the rocks'. We should bring a new picture from elsewhere and see if it produces a more adequate interpretation of fossil evidence."

My best example comes from Mayr when he concedes that there are valid objections to his theory, but rules out these objections on the ground that their proponents have not advanced a better suggestion: "... it is a considerable strain on one's credulity to assume that finely balanced systems such as certain sense organs (the eye of vertebrates, or the bird's feather) could be improved by random mutations. This is even more true for some of the ecological chain relationships (the famous yucca moth case, and so forth). However, the objectors to random mutations have so far been unable to advance any alternative explanation that was supported by substantial evidence."

4. *Theory comes before facts.* I used to think that scientists were led to formulate hypotheses and theories by their observation of phenomena, but this is not the case in biology. The practice there is to formulate a hypothesis and then to go looking for facts. Darwin himself operated on this basis. As Ghiselin says: "Only in the minds of journalists and metaphysicians do significant advances in science result from a determination to be 'objective' and to gather facts without any preconception as to what the facts may mean."

Such a practice has questionable effects on theories and hypotheses, but even more so on the phenomena. If they do not match the theory, i.e. if they seem to be inexplicable, they are likely to be swept under the rug. This has been done to ESP and the UFOs, and it has led the evolutionists to overlook or reject many less exotic phenomena that must ultimately be accounted for. Simpson acknowledges and even defends this practice: "... we are, quite properly, I believe, reluctant to accept the reality of a phenomenon, be it evolution or extrasensory perception, unless there is some plausible hypothesis as to its mechanism." He goes on to say that it is a trait of the scientific mind to behave thus: "... scientists rarely are psychologically capable of accepting a phenomenon as a fact and also accepting it as inexplicable. It is well for the progress of science that this is so, but it has led to many premature and to some bizarre theories." Simpson has a wonderful ability to make a virtue of necessity.

5. *Allow me to explain*. The above vagaries might not be worth mentioning if the biologists did not feel such a passion to explain. The pages of their own journals are full of attempts to explain thousands of questions and problems that they have conjured up, but at least these attempts are addressed to their colleagues and are suitably qualified and cautious. When they speak to the public, as in the pages of *Natural History*, they become less cautious and more authoritative. The photographs are beautiful and the research is painstaking, but always there is a little effort to show that Darwinism has the answers if you will only ask the questions.

There is a difference, however. It is not the full-time high-level evolutionists who purvey these explanations. It is popularizers or second-echelon researchers. The big leaguers are too prudent to expose themselves. They prefer generalities. Look at the major works of Simpson, Mayr, Stebbins, Ghiselin, Fisher, or Hardin, and note how they avoid specifics. They are never tempted into trying to explain the woodpecker's tongue, the archer fish, the narwhal's tusk, or other notable marvels. They prefer to emphasize industrial melanism, the trivial case where the peppered moth varies from black to white. This may have little to do with evolution, but it is safe ground.

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ACTUALISM IN GEOLOGY AND IN GEOGRAPHY²

The great geological controversies of the first half of the 19th century are usually described as ending in the rejection of catastrophism and diluvialism and the victory of uniformitarianism, or actualism as it is called on the Continent.

The names of HUTTON and PLAYFAIR in Britain, of VON HOFF in Germany and, somewhat later, those of PREVOST in France and LYELL in England are linked to the latter theory, whereas WERNER, CUVIER and BUCKLAND, among others, are quoted as protagonists of catastrophism.

Thus in a rather simplified picture the debate for instance on the origins of granite and basalt, Plutonism v. Neptunism, is linked to the controversy about the importance of 'causes now operating' and even to the question of subaërial denudation in valley formation.

Some authors stress the role played by a naive-realistic interpretation of the Bible: CHORLEY et al. (4, p. 97) even print parts of the "accepted version and chronology of the Old Testament" (Gen. 1, 1-7 and VII, 11—VIII, 14) and HOLMES mentions that Indian beliefs would have allowed much greater lengths of time for geological history (10, p. 44). It is clear from HOOYKAAS' (11, p. 170 seq.) account, however, that there was never any strict correlation between religious beliefs and scientific convictions. In the beginning of the 19th century, moreover the objections based on the length of time required by uniformitarian explanations were not founded on Biblical chronology only (cf. BAULIG, 1, p. 32). It is perhaps not superfluous to recall that even in the twenties of the present century there was a considerable discrepancy between the estimates of the age of the earth based on astronomical considerations and the results of the first radioactive age determinations.³

It cannot be denied on the other hand that religious beliefs did in some cases clash with the results of science; thus BECK⁴ mentions that VON BUCH was disturbed when his observations in Central France led him to a vulcanist theory on the origin of basalt. But he succeeded in reconciling his faith and his science as did BUCKLAND and SEDGWICK "when they contrived to sail out upon its (the diluvial tide's) retreating waters" (CHORLEY et al., 4, p. 124).

In many textbooks of geology and/or geomorphology, actualism or uniformitarianism is listed among the fundamental concepts of the discipline. Often is it only loosely defined, e.g. "the study of the present is the key to the past" (This is sometimes ascribed to HUTTON, but I have not been able to trace the quotation). In any case it is clear that this statement implies no more than that geology, like any science, is based on experience, for we can have experience only of the present. HUTTON is more explicit in the following quotation:

¹ Dr. W. J. JONG, Heemstede, Holland

The figures in brackets refer to the list of references at the end of the article.

² 'Actualism' is listed both in the *Oxford Dictionary* and in *Webster*, but only in a philosophical sense. The *Encyclopedia Americana* however, gives the meaning in geology, so that I feel justified in using the English form, without quotation marks or final 'e', though some ambiguity may result as we shall see.

³ And when agreement appeared to have been reached around 1940, the figures agreed on now seem much too low: UMBROGROVE, *Pulse of the Earth*, the Hague, 1942, mentions 2 to 3000 million years (p. 9).

⁴ *Vulkanisten und Neptunisten in Hessen* (Ber. z. dts. Landesk., 1961, Bd. 27, S. 105).

Before discussing the implications of uniformitarianism as a principle and as a theory we must deal with a somewhat intricate matter of vocabulary. The following quotation from TRICART and CAILLEUX (18a, p. 166) may serve as a starting point:

"En langue anglaise *actual* veut dire *effectif*, et non pas *actuel*, ni présent. Causes *effectives*, voilà ce que voulait dire LYELL, comme il ressort de son texte; il les opposait aux causes plus ou moins *imaginaires*, comme le déluge universel ou l'universelle fusion des roches ..."
 "Mais comment savoir si une cause est effective (*actual*), si ce n'est en cherchant à l'observer en action, de nos jours?". "Et ainsi s'explique que les traducteurs français aient pu, sans déformer gravement sa pensée, traduire *actual* par *actuel*" (author's italics).

But TRICART and CAILLEUX are in error: LYELL did *not* write 'actual causes', he wrote 'present causes', 'modern causes', 'causes now in operation'.

The French translators of the time quite correctly rendered this as 'causes actuelles', or 'causes actuellement actives'.⁷ As far as I could find, LYELL in the *Principles* used the word 'actual' in the sense of 'present' only once (13, p. 1), and I did not find an example of the use of 'actual' in the more common sense of 'real'.

There are some places in HUTTON though:

"In examining things which *actually* exist, and which have proceeded in a certain order, it is natural to look for that which had been first (*Theory of the Earth*, 1795, I, pp. 280-'81). "A theory, therefore, which is limited to the *actual* constitution of this earth, cannot be allowed to proceed one step beyond the *present* order of things" (ibid., both quotations after CHORLEY et al., l.c. p. 37, my italics).

I think it is evident from the context that HUTTON here uses 'actual' as a synonym of 'real', in accordance with modern English usage.

It is not difficult to find modern English examples of 'actual' in the sense of 'present'. The *Oxford Dictionary* quotes A. GEIKIE's *Physical Geography* as an example, where he speaks of nobody having witnessed an actual eruption of volcanoes on the Moon. And it is even easier to find instances, for example in psychological texts, of Continental languages using 'actuel' (aktuell, actuel), or derivatives, in the sense of 'real'. But this does not do away with the fact that LYELL used 'modern', 'present', and not 'actual' and that therefore he did not only mean 'effective causes' as opposed to 'imaginary causes', but in fact 'modern causes' as opposed to 'ancient causes'.

Nor can it be maintained, as HOLMES (10, p. 44) appears to do⁸, that the Continental use of 'actualisme' ('Aktualismus') only implies 'reality', 'effectiveness' and does not refer to 'present causes'.

HOOYKAAS (11, p. 3-14) attempts to classify the concepts involved in a double dichotomy that may be rendered thus:

kind	intensity		
	not different		different
	not different	uniformitarianism	actualism
	different	CAYEUX's 'causes anciennes'	catastrophism

⁷ See for instance: Engl.: "present effects" (p. 281); "influences actuelles" (p. 874); "agents of change, now operating in nature" (p. 20); "causes de changement actuellement actives" (p. 25) (LYELL, l.c.).

⁸ On the continent the term *uniformitarianism* never won favour but was gradually replaced by *actualism*, which conveys much more appropriately the real meaning of HUTTON's inspired appeal for 'actual causes'.

He did not, however, maintain this distinction throughout his book, and in the Glossary it is stated that in modern Anglo-American usage 'uniformitarianism' and 'actualism' are synonymous. Though in the context of HOOYKAAS' book the distinction is useful and his regret, as expressed in the Preface to the 2nd edition, at not having made it consistently, is understandable, I do not think it will be of any practical use outside historical discussions. After all, HOOYKAAS is a historian of science, not a geologist, as RUTTEN has remarked (15, p. 8).

I shall not make this distinction and use both terms as synonyms. The following quotation from a recent publication might perhaps be cited in support (HACK, 8, p. 87):

"This kind of analysis is uniformitarian in its approach, for it attempts to explain landscapes in terms of processes and rates that are in existence today and therefore observable. It recognizes that processes and rates change both in space and time".

The last sentence speaks for itself, in HOOYKAAS' classification this is actualism, not uniformitarianism.

We now return to modern definitions of actualism or uniformitarianism. It figures first among the fundamental concepts listed by THORNBURY (17, p. 16) as "the great underlying principle of modern geology":

"*Concept 1*: The same physical processes and laws that operate to day, operated throughout geologic time, although not necessarily always with the same intensity".

But COTTON (5, p. 1) is more careful:

"It is recognised that the present condition of the earth's surface is due in great part to the long-continued action of processes still in operation... Nowadays such pronounced uniformitarian views, as they are termed, are regarded as extreme. The importance of physical geology hinges, nevertheless, on the acceptance of the present as 'a key to the past'."

Thus COTTON distinguishes between the inevitable methodological principle and a generally accepted theory about the not too distant geologic past, the imprints of which are still to be recognized in present land forms. THORNBURY, however, lumps together a methodological postulate and a theory about processes 'throughout geologic time', though further on he writes:

"*Concept 6*: Little of the earth's topography is older than Tertiary and most of it is no older than Pleistocene" (l.c., p. 26).

which means that by far the greatest part of geologic time, 98 to 99 %, does not concern him.

HOLMES is more sweeping:

"The principle that the same processes and natural laws prevailed in the past as those we can now observe or infer from observations" (l.c. p. 44).

And RUTTEN (15a, p. 7):

"They (i.e. the words uniformitarianism and actualism) express the assumption of a certain continuity of physical processes in geological history... Once this assumption is accepted, one may study actual (here: present! W. J.) processes and extrapolate these into the geological past to interpret our factual findings in forming a genetical, historical picture".

Here again the confusion of a methodological postulate and theories, 'pictures', about the past. With reference to the latter VON ENGELN says that uniformitarianism is only tenable "if numerous qualifications and exceptions are kept in mind" (6, p. 82).

To the Dutch reader it might be interesting to compare ESCHER's views as expressed in two summaries of geology intended for a larger public. Around 1938 he writes (my translations):

"After having been tested by geologists all over the earth for over 100 years, this doctrine (i.e. actualism) retains its validity". (7a, p. 254).

But in 1948 the 'qualifications and exceptions' referred to by VON ENGELN are much more explicit:

"That LYELL's principle of uniformity cannot be applied to the first 2500 million years is certain. And for the remaining 500 million years it is also certain that actualism does not apply to the energy per unit of time involved in geologic processes... Periods of strong endogenic forces have alternated with periods of weaker activities... And this went hand in hand with alternations in the intensity of destructive exogenic forces... But for the 500 million years of the last, sixth, part of our earth's history LYELL's principle of uniformity retains its value in so far as it concerns the nature of the geologic processes" (7b, p. 81).

So we are dealing with at least three aspects of actualism:

1. Reliance on direct observations, or at least on trustworthy evidence of earlier observations. "Hingehen und gucken" ⁹ is not always possible, alas!, and we are often dependent on hearsay and circumstantial evidence.

2. The methodological postulate of uniformity in a wide sense. In essence this was already taught by WILLIAM OF OCKHAM in the 14th century: "entia (principii) non multiplicanda praeter necessitatem", "principles (causes, laws) are not to be introduced without necessity," 'without necessity' being the operative words. This 'necessity' has been the issue in the various debates: whether we could, or could not, do without 'ancient', 'different' causes. And even HUTTON himself warned:

"we are not to limit nature with our imbecility or estimate the powers of nature by the measure of our own" (*Theory of the Earth*. Roy. Soc. Edinb. Trans., I, 1788, p. 280. after CHORLEY et al., l.c., p. 42).

3. A picture, a model, i.e. a theory, about the geologic past and the forces operating then. The evidence from present observation of forces now operating is that they vary both in space and in time, but also that those variations are not the same for the various processes that are modelling the earth's surface. RUTTEN (15a, p. 65) distinguishes between endogenic and exogenic processes, but the matter is more complicated: for some exogenic forces extrapolation from present observations is more hazardous than for others (weathering, gravity).

In some of the debates still another aspect comes in. Does 'uniformity of intensity' imply uniformity of the rate of change, i.e. of the amounts of energy involved, or does it also cover changes in these amounts according to some law? That is: do we assume the first or the second difference quotient of energy by time to be constant or even zero? HOOYKAAS (11, p. 152) points out that it even might be (and is) taken to mean that the energy involved could be represented by some ill defined periodic function of time, what he calls Neo-Huttonian theories.

But this is clearly a matter of scale, both of the time and the area over which we average. And because the necessary data are lacking it remains highly speculative. As the period of a hypothetical 'pulse' of the Earth' (UMBROVE, cf. note on p. 32) clearly

⁹ RUTTEN, l.c. p. 8.

exceeds the span of time open to geomorphic enquiry by a factor 4 or 5, we need not discuss its reality any further.¹⁰

Recently RUTTEN has distinguished (15a, p. 63) between a pre-actualistic period in the history of the earth, characterized by an an-oxygenic atmosphere and an actualistic period, covering only the last 1.7 to 1.2 Gy!¹¹

Actualism, at least for exogenic processes (see below) is thus confined to the Phanerozoic and the later Pre-Cambrian. VAN BEMMELEN, however, favours another division.

During a first, anhydric, phase covering the history of the earth prior to 3.5 Gy ago, actualism was not valid. For a second phase, from 3.5 to 0.5 Gy, actualism is applicable only to a limited extent and only for the third phase actualism is accepted. (2, p. 32, fig. 5). However, the criteria applied by VAN BEMMELEN are of an entirely different nature, they are concerned with endogenic forces: archeo-vulcanism giving way to crust formation and consolidation of the primeval continents, in their turn succeeded by continental drift and disaggregation.

RUTTEN, who stresses the actualistic nature of endogenic forces even during the existence of a pre-actualistic atmosphere (l.c., p. 65) does not explicitly state whether he thinks this also holds for the oldest periods of the earth's history, but it is evident that 'pre-actualistic' has a different meaning for the two authors.

In the Dutch version of his book RUTTEN incorporated the theories of BERKNER and MARSHALL (15b, pp. 108 seq., and 3, pp. 415 seq.). These authors have worked out a model for the change of an an-oxygenic atmosphere to our modern atmosphere by the oxygen production of living organisms. This model is characterized by the presence of two critical levels. The first is attained when the oxygen content is at 1 % of the present level, at the beginning of the Paleozoic, explaining the 'evolutionary explosion' some 6 My ago. This level of oxygen content would offer sufficient protection to life in the upper layers of the oceans. But not until a 10 % level had been reached and a thicker shield of ozone had been formed:

"Newly evolving organisms can spread in great profusion over the land surface; resulting in a further acceleration in the processes of photosynthesis and respiration" (l.c., 418, 2nd. col.).

This second critical level is placed at the end of the Silurian, some 4 My ago. By the end of the Devonian, oxygen content would have risen to, if not exceeded, the present level. And as oxygen production is directly related to carbon dioxide consumption, this could not but have influenced the overall temperature of the earth. RUTTEN tends to stress the near constancy of the average temperature over the globe (l.c. p. 120 seq.), in the Dutch version (l.c. blz. 187) a figure of less than 10° C is mentioned for the mean temperature change between (or: in?) Ice or Heat Ages. This assumption rests on the survival of life, but it is clearly a matter of taste whether a temperature change of this amount (in the global average!) can be reconciled with 'uniformity'.

But BERKNER and MARSHALL (l.c., col. 3) write:

"Diminution of this (the 'greenhouse') effect due to reduction (*sic*) of carbon dioxide during the Carboniferous might have lead to the extensive ice ages of the Permian period... Photosynthesis would fall sharply as organisms were constrained by the changed climate, leading to an abrupt, catastrophic reduction in oxygen concentration". "One would like to know whether those great reptiles of the Cretaceous attempted to acquire a greater lung capacity before they disappeared suddenly from both land and sea."

¹⁰ Cf. HOOYKAAS, l.c. p. VIII and HACK, l.c.

¹¹ Gy = Giga-year, i.e. 10⁹ years, and My = Mega-year, 10⁶ years, thus we avoid the ambiguity of 'a billion' (these figures are taken from the Dutch version, 15b, p. 176).

It would certainly appear that these American physicists are no more averse from 'catastrophic' speculations than early 19th century geologists! And surely GENERELLI's wish for a history of the earth "senza violenze, senza finzioni, senza supposti, senza miracoli" ¹² expressed in the Academy of Cremona in 1749, is no more met by these modern theories than by CUVIER's or BUFFON's.

And then we have not even mentioned cosmological theories. The 'big bang' was a 'catastrophe' if ever there was one; and LIBBY's hypothesis of the "catch of an anti-matter-meteorite", in Siberia in 1908, certainly would have been regarded as 'superstition' ¹³ by more modern uniformitarians. Since radio-astronomy of the quasars seems to lead to ever greater ages for the universe (latest figure: 10 Gy) one wonders if geology still has the monopoly of 'asylums for hypotheses': the interior of the earth and 'unlimited time', ¹⁴ and whether outer space does not qualify as such! On the other hand: does not HOYLE's 'steady state' theory of the universe recall HUTTON's well known "no trace of a beginning, no vestige of an end"? ¹⁵

Both critical levels of BERKNER and MARSHALL lie well within RUTTEN's actualistic period. In their theory biogenesis sets in with oxygen at 0.1 % of the present level, which can be accounted for by the dissociation of water vapour alone, ¹⁶ and in itself is not due to life. RUTTEN's an-oxygenic atmosphere might then be better described as oligo-oxygenic. In RUTTEN's theory the change from pre-actualistic to actualistic atmosphere, and weathering, took place some 1000 million years earlier than in BERKNER's and MARSHALL's, but on the strength of the earliest datings of 'red beds' RUTTEN is inclined to date their first critical level back by that amount. To account for the sudden appearance of numerous fossils at the beginning of the Cambrium he invokes a change in the carbondioxide cycle: as a consequence of increasing CO₂-consumption, the pH of the ocean water was lowered, and calcareous 'shells' could be formed. (15b, blz. 172-4).

On the other hands he pays little attention to the second critical level, related to the appearance of terrestrial vegetation. But this, as ERICH KAYSER, BAULIG, and TRICART and CAILLEUX, i.a., have pointed out (cf 1, p. 34; 18a, p. 160; 18b, p. 398) meant the end of the 'biologic desert'. The absence of vegetation, and therefore of a soil, led to an intense mechanical weathering and active erosion, both by running water and by wind, without these necessarily having been accompanied by signs of aridity, such as evaporites.

We can even go further: soil formation in the modern sense could not set in until terrestrial vegetation included a great variety of herbaceous plants and grasses. And according to the Russian soil scientist WILLIAMS it was only then that lime fixation in the soil and the plant cover, such as we now know from the 'pedocal' soils, could take place. ¹⁷

Summarizing our review for the periods prior to the Quaternary, we might distinguish

¹² "Without violence, without fictions, without suppositions, without miracles". After ESCHER, 7b, p. 81.

¹³ Cf. HOOYKAAS, 11, p. 44.

¹⁴ P. PRUVOST, *Les refuges de l'hypothèse en géologie*. 1951, Congr. int. phil. sc. VII.

¹⁵ VAN STEENIS has drawn a similar parallel between "steady state" and land bridges on the one hand, and "the great explosion" and continental drift on the other (*Blumea*, II, 1962, p. 315). This is quite in the spirit of HUTTON and LYELL. It only came to my notice after going to press.

¹⁶ This is limited by ultra-violet absorption in the ozone produced (UREY-effect).

¹⁷ WILENSKY, 19. S. 116. Both names, having been transliterated from the Russian, appear in different spellings: WILJAMS, VILJAMS, VILENSKII.

(from old to young) periods characterized by:

no water, no oxygen, no life	up to perhaps —3.5 Gy ¹⁸
water, no oxygen, no life	up to, at most, —2.7 Gy
water, no oxygen, aquatic life	up to between —1.7 and —1.2 Gy
water, oxygen, aquatic life only	up to —0.4 Gy = —400 My
water, oxygen, terrestrial life, no grass	up to around —100 My
water, oxygen, grass	up to Recent

And only for the last of these periods we can say, without important qualifications, that 'forces now operating', in LYELL's sense, were operating then. Only in this period actualistic conditions may be said to have prevailed, not only for elementary physical and chemical processes (turbulent flow, oxidation, might be cited as examples, cf. TRICART and CAILLEUX, 18a, p. 168) but for those complex processes and systems studied by geology and geomorphology.

"The geologist looks at the present that he may interpret the past; the geographer looks at the past that he may interpret the present."

This is how Sir HERBERT MACKINDER in 1887 characterized the distinction between the two disciplines mentioned in the title of this article (14, p. 146). This might be amplified, in the spirit of Dutch geography and certainly also of MACKINDER, to read: the present of the geosphere, as an environment for Man, and therefore orientated towards the future.

Among Soviet geologists and geographers in the Academy of Science (and recently also in the Czecho-Slovak Geological Service) it has become the vogue to speak of the 'Anthropozoic' instead of the Quaternary. (Even 'Psychozoic' has been suggested).

American geologists and geomorphologists, following FLINT, equate the Quaternary with the Pleistocene. Each of these expressions stresses one of the most characteristic features of the last 1-1.5 My¹⁹ of the earth's history: the appearance of the Hominids and the climatic fluctuations of the Ice Ages. Both were known to LYELL in his old age and he felt neither of them to be in conflict with uniformitarianism (cf. HOOYKAAS, 11, p. 29 and p. 115). And yet both released processes that had not existed before, or at least, in the case of the Ice Ages, of which no earlier evidence has been conserved.

The geologic Recent, "an exceptional period" as KUENEN called it in his inaugural lecture (Groningen 1939), "the warmest interglacial of the most glaciated geologic era" as CORBEL²⁰ says, would not seem the most appropriate for unqualified extrapolation backwards in time. And surely there is serious reason to doubt that "the thing that is, is the thing that has been and that shall be" (ASA GRAY, cf. HOOYKAAS, 11, p. 100). Therefore, even for the most recent 100 My we have to take into account that 'forces now in operation' may not have been in operation in the past while 'ancient forces' are no longer active at present.

Of course, one can always posit that there has been 'no appreciable' change over 'the earth as a whole' but without more, and especially more quantitative, data than we have now, this assertion is, as a Dutch economist (PEN) has put it: "merely literature". And we are not concerned with the earth as a whole, a planet in the solar system, but with

¹⁸ RUTTEN's figures, 15b. For comparison: granites from French Guyana have been dated at over 4 Gy and equally a Pb/Sr date of 4.5 Gy has been reported for a periodite from St. Pauls' Rock in the Central Atlantic (New Scientist, Jan. 14th, 1965).

¹⁹ By 1976 standards, this figure could be doubled. (Author's note to the second printing)

²⁰ *Neiges et glaciers*. Paris 1962, p. 197.

geologic and geographic regions, with areal divisions of an unbounded surface of 500 million km², whether this 'surface' is assigned a 'thickness' of some tens of kilometers or tens of decimeters (1 dm = 0.1 m).

To give just one example we'll look at conditions prevailing during the Würm in Central and Western Europe.²¹ With the ice margin near the German Bight and the tree line in Southern France the broad belt of periglacial landscapes presented conditions that have now disappeared from the earth. For, the Pole being near its present position, the radiation climate did not differ from the modern and yet a large part of this belt was underlain by permafrost and the albedo need not have been the same: no forest, more snow. Eolian activity was strong and low parabolic dunes developed in regions with no, or only scarce, vegetation cover. Of course we need not doubt that the relation between wind velocity and tractive force, determining sand transport, was more or less the same as now, but the resulting forms were different from those in the Sahara or on our modern beaches.

Actualism can be taken to mean no more than that both geology and geography are empirical sciences, and that, since order is one of the central concepts of science, to understand either the past or the future we have to start with the assumption of *some* underlying uniformity, so as to enable us to predict on the one hand what we'll find when looking for confirming evidence (in geology), or future developments (in geography) on the other hand. But all the time we must be prepared to revise our assumptions of the nature of this uniformity. If this is what actualism is taken to mean, it certainly remains a basic tenet of both geology and geography, but not of these sciences alone; it is basic to all science.

But if by actualism we understand, as is often implied, that no other processes than those now to be observed have operated in the past, so that the present can serve as a model for the past (or for the future), then it is different. For each of these processes we'll have to judge carefully, whether we are justified in supposing it to have been operative in some period of the past. And this applies even for periods in so recent a past as the Late Glacial. The case for uniformitarianism is different, not only for endogenic and exogenic processes, but also for those exogenic processes that are dependent on climate and those that are not. And different again for those that include biotic influences and those that are largely independent of life.

If we now turn to geography in particular, we must agree with TEILHARD DE CHARDIN that:

"ultimately neither earth nor man can be fully understood except with regard to the marvellous sheet of humanized and socialized matter, which, despite its incredibly small mass and its incredible thinness has to be regarded positively as the most sharply individualized and the most specifically distinct of all planetary units so far recognized" (16, p. 103).

The appearance of Man, at the end of the Tertiary, meant an even more "revolutionary transformation" than that "major alteration" when the Earth turned "chlorophyll-green" in the early Paleozoic (*ibid.*). But this transformation also introduced a new geologic and geomorphic agent that is certainly operating now and just as surely did not operate in the past. It has distorted our estimates of the effectiveness of other agents in the past: it is well known that in the more "humanized" zones (TEILHARD, *l.c.* p. 110, cf. also: TRICART and CAILLEUX, 18a, pp. 186-88) erosion, transport and sedimentation have been influenced by Man. (cf. JÄCKLI, 12, S. 94).

²¹ J. BÜDEL, *Erdkunde*. 1959, S. 297.

It is not correct, from the point of view of geology and geomorphology, to stress only the destructive character of Man's influence. For the material from the denuded slopes is deposited in the flood and piedmont plains and in the great deltas: not only Egypt is 'a gift from the river.' And even the material in solution, greatly increased in amount by the inevitable 'leaks' from the plant-soil-cycle to the larger geologic cycle, caused by Man's agricultural activity, is accumulated in the oceans.

But from a geographic point of view, focussing our attention on the environment, it does appear that so far the effect has been prevailingly destructive. 'Soil erosion', 'accelerated erosion' and 'man-induced' or 'anthropogenic erosion' are more or less synonymous. Nevertheless, signs of constructive 'transformations of nature' are not entirely lacking. Not so much in the great flood control and irrigation works, that have often had unsuspected side effects (lowering of the level of the Caspian, valley cutting, salinization, changing ground water levels etc.). But mainly in those most humanized areas where old established agricultural cultures have succeeded in establishing new equilibria, differentiating the elements of the landscape more sharply: "heterogenizing" it, we might say. Here too Man has succeeded in stopping, or at least reducing, the leaks of plant nutrients to the oceans and in adding transferred, or even man-made, fertility (nitrogen fixation from the air). The latter process is related to another development.

Ever since the Neolithic man has been burning fuel, destroying and changing the vegetation cover, building towns (CHILDE's "urban revolution"). But only since the agricultural and industrial revolutions of the late 18th and early 19th century "owing to the coincidence of a sharp demographic jump and the incredible progress in intercommunication" (TEILHARD, l.c. p. 111) urbanization has spread over the whole earth.

A town, an urbanized area, is not merely a concentration of population. As a physical part of the landscape it has been called "a flux of matter and energy". It is a source of heat and of air pollution, a wind break and a source of waste production, a load and a waterproofed surface. And it has released processes that even compared to LYELL's time might well be called 'post-actualistic'. Neither the present salt content of the Rhine, for example, nor the ^{14}C content of modern wood can be used for extrapolation into the past, and whether we can project the processes involved into the future remains an open question. The influences on mesoclimate are perhaps best known, but whether the addition of carbondioxide through industrial combustion also affects the radiation balance and thereby the macro-climate, ultimately depends on the effects of the oceans and/or the vegetation cover as buffers for carbondioxide. Locally air pollution has led to man-made biological deserts, but we still know very little of its less spectacular effects on vegetation, on weathering, on radiation, though London and Los Angeles offer warning examples.

Urbanization affects run off, and thereby infiltration. Consumptive water use, the difference between precipitation and run off (including infiltration) in urban areas is not necessarily less than in agricultural areas. Most of these processes only affect limited areas, but even now they are not confined to the built over area itself: sedimentation in Lake Zurich shows a change from white calcareous lake sediment to black anaërobic mud since the beginning of this century, due to pollution (and fertilization) of the surface waters which has led to a sharp rise in plankton production. And with the spreading of urbanization such processes may be expected to spread to ever wider areas.

KENNETH HARE recently called for a renewed interest of physical geographers in the natural environment (9, p. 109). I would like to endorse this plea and to extend it to the physical geography of an environment that is becoming 'natural' for an ever greater part of mankind. After all, if the urbanized environment is man-made, so is a field, a meadow or a wood.

A provisional assumption of uniformity, an actualistic approach in a post-actualistic environment, is indispensable, whether it is studied as a key to the past or as a key to the future. But in this, if anywhere, we should be aware of the need for constant revision of this very assumption.

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GEOPHYSICAL TIME SERIES AND CATASTROPHISM

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Introduction

The old goal of a great many Egyptian rulers and governments to harness the Nile River and 'make use of the Nile water to the fullest possible extent' (Hurst *et al.*, 1965) has led to extensive studies of hydrological records of the Nile River and, in particular, of the long series of maximum flood stages recorded on the Roda gauge in Cairo which dates back to 641 A.D.

Since about 1936 these studies were conducted by H.E. Hurst and were aimed at finding a storage reservoir capacity that would be necessary to make the mean annual discharge of the Nile River constant and equal to its long-term mean. 'The method for doing this is well known and consists in taking the departures of the yearly discharges from the mean in order and forming their continued sums. The difference (range), R , between the maximum and minimum of these sums is the storage which would have been required to maintain the mean discharge throughout the period' (Hurst *et al.*, 1965). The method is illustrated in Figure 1. In order that the values of R_n corresponding to a given series length n could be compared for different time series, the range is normalized by the standard deviation S_n of the series to form a variable R_n/S_n called the 'rescaled range'.

Hurst (1951) found that for a random series

$$E \left[\frac{R_n}{S_n} \right] \sim n^{0.5} \quad (1)$$

where E signifies the arithmetic mean and the sign $\sim$ indicates proportionality. This result was confirmed by Feller (1951) and was later shown to hold asymptotically not only for purely random processes but also for all the common types of stationary stochastic processes. It was therefore expected that for large n the plot $\log E (R_n/S_n)$ versus $\log n$ (let us call it the 'range plot') of empirical time series should have an asymptote with slope equal to 0.5. It thus came as a great surprise to theoreticians when Hurst showed that the range plot of the Roda gauge data as well as range plots of long series of precipitation, river discharges, annual

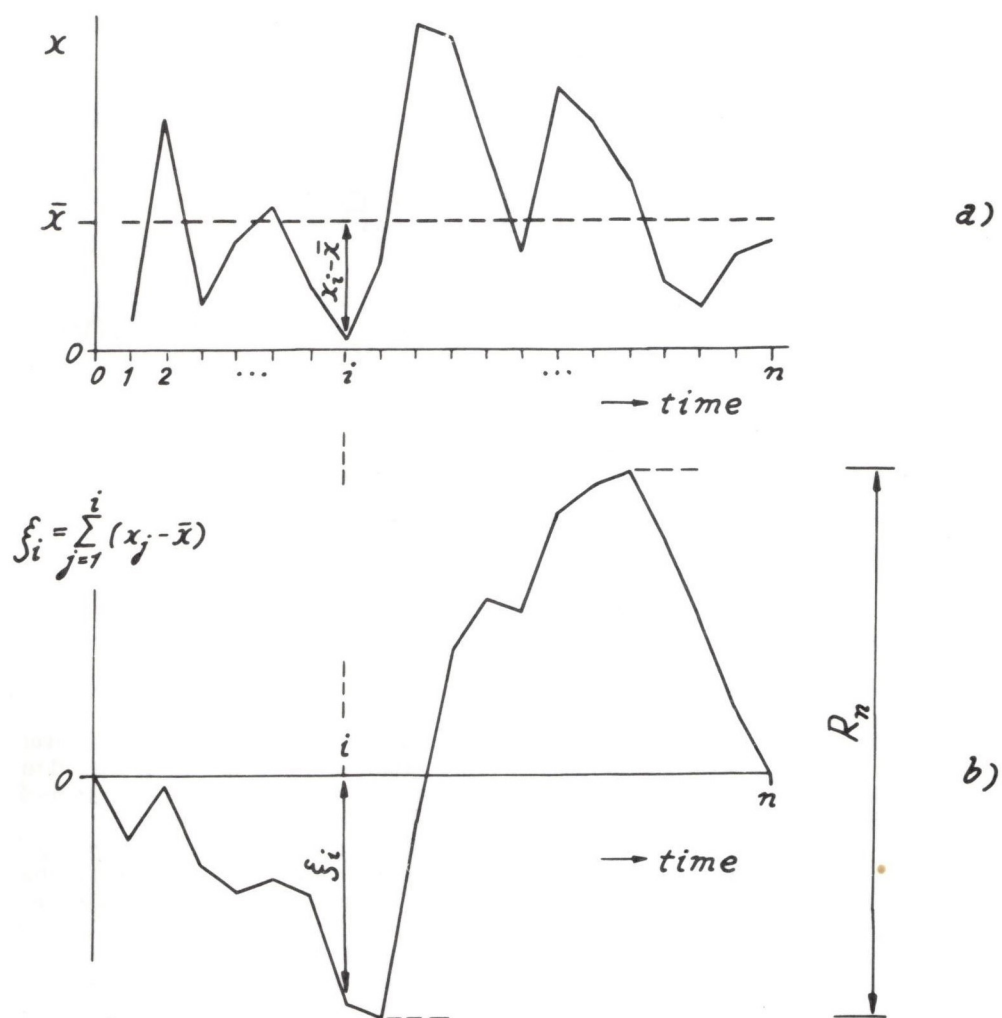


Fig. 1 a) Time series of variable x
b) Time series of cumulative departures from the mean x

temperatures, and pressures, annual sediment deposits (varves) in lakes, and tree ring thicknesses exhibited a relationship

$$E\left[\frac{R_n}{S_n}\right] \sim n^h \quad (2)$$

where h was close to 0.7. Similar empirical findings were later made with regard to economic and other historic time series.

The failure of the exponent h to accord with theory underlying eq. (1) has been labelled the Hurst phenomenon. It has long resisted an adequate theoretical explanation and geophysicists and mathematicians still have not reached agreement as to its cause.

One theoretical model that accounts for the Hurst phenomenon is a stochastic process called fractional Brownian noise (fBn) put forward by Mandelbrot (1965) and later developed by himself and his collaborators (a detailed bibliography is given, for instance, in Klemeš, 1974). The fBn is a time series, each term of which, $x(t)$, has been constructed as a weighted sum of an infinite number of Gaussian random quantities, $\varepsilon(t-1), \varepsilon(t-2), \dots, \varepsilon(t-\tau)$; $\tau \rightarrow \infty$ where the weights are τ^{-k} , with $0.5 < k < 1.5$. The fBn can possess any value of h observed in historic series and it (or rather its approximations) has been successfully used as an operational model for hydrologic series. The problem is, however, that this mathematical model implies that the modelled physical process has an infinite memory of a peculiar kind: the formation of each new term of the series requires that all the past values be available (i.e. stored) since their original values are repeatedly used and modified by different coefficients (weights) in every step. Such a mechanism can be easily visualized in economic time series where the original values of variables (prices, volumes of stock, etc.) are preserved in the book-keeping process, and in some biological processes where the long-term memory could be related to the mechanism of genetic coding. It, however, does not seem likely that the model has universal validity, especially in geophysical processes which tend to have the so called Markovian property, it means the past history of the process affects its present state through its cumulative effect which is fully reflected in a small number of the most recent states of the process. For instance, the rate of outflow from a lake depends on the instant elevation of its water level and the slope of the surface, and it is immaterial through what sequence of past events their current values have been reached. It is hard to visualize a physical mechanism through which, for instance, this month's mean temperature could have been directly influenced by that of, say, May 1950 or 1850.

The present author (Klemeš, 1974) has suggested two different processes that account for the Hurst phenomenon and whose mechanisms are compatible with geophysical processes. One of them is a compounded cumulative process arising in a series of semi-infinite storage reservoirs where output from one becomes the input into the next. This process, although quite common in hydrologic and related processes, will not be discussed here as it has no bearing on the concept of catastrophism. It is the other process that can be of interest in this connection.

Process with time-variant central tendency

Consider a historic record of a time series describing fluctuations of some geophysical variable. For the sake of simplicity the variable concerned will be one defined on the yearly basis, for instance the annual precipitation total or maximum air temperature at a particular geographic location, date of freeze-up of a particular lake, etc.

In trying to develop a mathematical model for such series we must first introduce certain assumptions which may be based on our knowledge (or the lack of it) of the physical laws governing the process, on the geometry of the historic series, on the tools available for the analysis. In any case, these assumptions serve to simplify the complex reality and to make it fit into our preconceived schemes called 'organized system of knowledge'. It should be pointed out that these assumptions are to a great extent arbitrary.

The first assumption will probably be one concerning the stochastic nature of the series. It implies an admission on our part of not being able to predict the future values of the series accurately. The degree of uncertainty involved can be ascertained only by making predictions and testing their accuracy. To make predictions, or estimates (they need not relate to specific values of the variable but to their dispersion corresponding to a given probability), specific assumptions, or sets of assumptions, about the pattern of the series behaviour are necessary.

Here we must first decide the following fundamental question: does the series represent fluctuations within some firm, stable general pattern, or does the pattern, the master plan itself, change? In other words, is the series a stationary one or a nonstationary one?

The geometry of the historical record can seldom provide a clear-cut answer. For instance, almost any series can be equally well visualized as either a sequence of large fluctuations about a constant mean or a sequence of small fluctuations about a sharply fluctuating mean (Figure 2). Mathematical convenience dictates the acceptance of the stationary hypothesis whereas the physical evidence testifies to the contrary. It seems to make little sense to assume, for instance, a constant mean discharge at a certain point of a river if we know that the slope of the river and the topography of the basin undergo permanent changes due to erosion and sedimentation; it would seem to be even less reasonable to assume a constant mean discharge over a period containing a glacial age during which the river ceased to exist.

In this connection the following two things should be noted, (1) that the failure to account for the Hurst phenomenon is observed in the common types of stationary stochastic process which, by definition, all possess a constant long-term mean, (2) that the variables R_n and S_n , on the basis of which the Hurst phenomenon is defined, are both functions of deviations from a constant long-term mean $\bar{x}$.

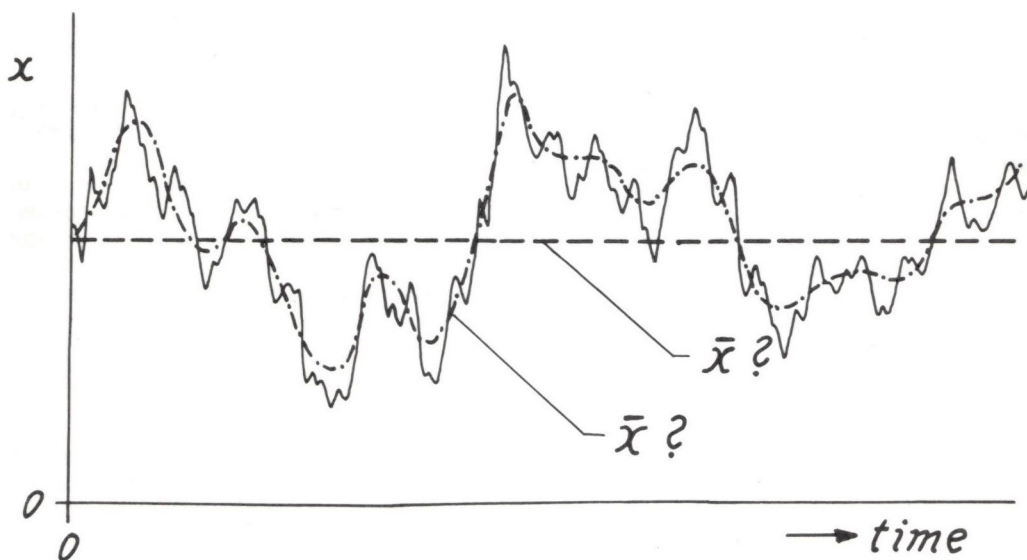


Fig. 2 Illustration of constant and time-variant mean of a time series

These two observations have lead the author to experiments with processes which have a time-variant mean. It was found that the simplest type of process exhibiting the Hurst phenomenon is one where the mean has constant value $\bar{x}_i$ within an epoch of some finite duration t_i , then suddenly takes on a different value $\bar{x}_j$ which is maintained during an epoch of duration t_j , after which another sudden change takes place, etc. Another type of process which leads to almost identical results is one where the values of the means $x_i, x_j, \dots$, within epochs $t_i, t_j, \dots$, are not constant but vary as linear trends. Sample series of these two processes are shown in Figure 3.

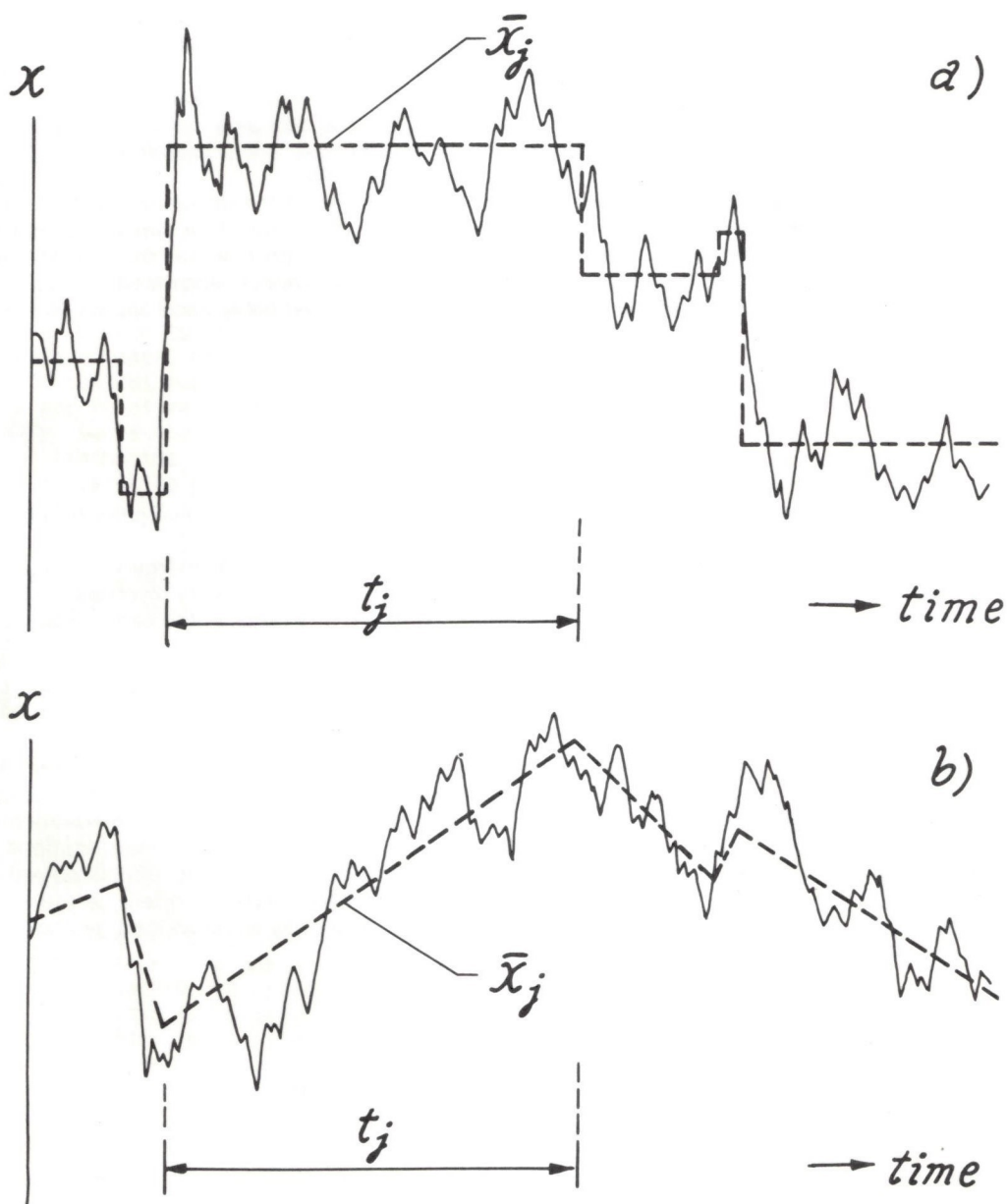


Fig. 3 Two types of constationary processes exhibiting the Hurst phenomenon

- a) constant means within epochs
- b) linearly changing means within epochs.

When such processes are treated as if they were stationary, i.e., if a fictitious constant long-term mean is assumed and all the deviations entering R and S are taken from this value, both processes exhibit the Hurst phenomenon. If, however, the deviations are properly taken from the actual time-variant mean, then the Hurst phenomenon disappears and the exponent h in eq. 2 approaches asymptotically the value of 0.5. This indicates that the Hurst phenomenon may arise from an incorrect interpretation of a process, specifically from assuming stationarity when the process is in fact nonstationary in the mean.

The type of probabilistic distribution of the mean $\bar{x}$ itself and of the deviation $x - \bar{x}$ does not seem to be important for the result nor does the stochastic structure of the process within a single epoch. The crucial thing is the distribution of epoch length t . It was found that the probabilities of epochs must be roughly inversely proportional to epoch lengths which implies that long stationary-looking periods exist but are very rare. A reasonably good approximation of the probability density of t appears to be

$$g(t) = \frac{\bar{t}}{\bar{t} - 1} t^{-\frac{2\bar{t} - 1}{\bar{t}}} \quad (3)$$

where the mean epoch length $\bar{t}$ can vary from 1 to ∞ , its different values giving rise to different values of the power h in eq. 2.

In physical terms the central tendency of a series reflects the overall energy level of the process. Physical causes of sudden changes in this energy level can be very diverse and can range from man-made effects on the environment, through processes in the earth crust, to extraterrestrial phenomena.

Conclusions

The purpose of this paper has been to point out that the behaviour of time series of long historic records of geophysical processes is compatible with the concept of catastrophic evolution, more specifically with the occurrence of sudden changes in the environment which take place at irregular time intervals. While it does not exclude other interpretations, for instance the presence of long persistence or storage effects which can be associated with certain types of processes, the concept of time-variant central tendency offers, in the author's opinion, the physically most plausible explanation, especially as far as long geological time scales are concerned.

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After a banquet in ancient Greece, guests received some eatable or drinkable gifts, APOPHORETA- 'things to be carried off', to take home. On this page a column is started in which provocative hypotheses are presented. They may serve to stimulate thought and research - and the correspondence for the 'Comments' column of this journal.

APOPHORETA ΑΠΟΦΟΡΗΤΑ

Early this year, professor Doeko Goosen in Enschede, Holland, told me that there was something odd about the iron content of the early-Holocene coversands of the Netherlands. These sands are thought to have been formed through a combined fluvial and aeolian activity. But in many of their soils, the amount of iron is much too high for such an origin. Moreover, the present loss of iron by seepage water, observable along many ditches, demonstrates that the original iron content must have been higher still. Weathering of minerals (loss of silica and relative accumulation of iron) does not satisfactorily explain this anomaly. Could the iron have come from above, as a sort of ferruginous loess?

A few months later in Mato Grosso, Goosen's remark led me to look more closely at laterite profiles. I noticed an inch-thick layer of hard laterite between two layers of unconsolidated gravel; its undersurface was smooth: it had obviously been formed prior to the deposition of the top gravel. I traced the layer for several kilometers, and later found it in places tens and even hundreds of kilometers away, on different geomorphological levels. The only possible explanation for these observations seemed aeolic precipitation on a barren, moist surface.

As I had only a vaguely remembered textbook knowledge, I started, back in Rio, a literature search on ferralitic soils (laterite, bauxite, terra rossa). I found that in the pages of Economic Geology a lively discussion on bauxite formation had been in progress for several years, between proponents of the residual theory, the alluvial theory, and the volcanic ash theory. I also discovered, from the official geological literature, that:

- 1- many fossil bauxite deposits were formed during a few (three or four) short periods in the Cretaceous;
- 2- in the Tertiary, a fossil ferralitic soil, including important bauxite deposits, formed probably during only one short period, in the early Eocene;
- 3- 'modern' ferralitic soils are not developing any more. Hematite, which gives the red colour to these soils all over the tropics, is not formed at soil temperatures in humid surroundings. Erosion of these soils has been underway for about 10.000 or 12.000 years. They are also fossil, the formation of ferralitic soils is a non-actualistic process. (Ferruginous soils on somewhat older, p.e. Pliocene surfaces may well be younger: if we drop a few coins on such a surface now, no archaeologist will conclude that these coins date from the Pliocene).

Thus, as far as we can tell from the geological record on the continents, the following may have occurred: in the early Eocene, heaps of ferralitic dust fell from heaven over a period of a few weeks or months; then everything was quiet for about fifty million years, until at the close of the Pleistocene another 'ferrallite event' took place. This iron dust must also have fallen in the oceans, and the red deep-sea clays would constitute the oceanic equivalents of the continental red soils. Do Glomar Challenger data support this hypothesis?

in this issue :

- 1- Catastrophist geology / Johan B.Kloosterman (=circular)
- 2- Comments / Derek V.Ager, Aart Brouwer, George Cuvier,
V.Axel Firsoff, Pierre Routhier, Harold
Tresman, and others
- 3- Scientific censorship and thought control / Horace C.Dudley
- 4- Whimsical aspects of scientific theory / Norman Macbeth
- 5- Actualism in geology and in geography / W.J.Jong
- 6- Geophysical time series and catastrophism / Vít Klemes

in forthcoming issues :

- Doeko Goosen on mass-wasting in level areas
- Alistair Pitty on the formation of peneplains
- Johan Kloosterman on ways of thinking in geology
- Charles Musès on patterned mutation
- L.I.Salop on glaciations and crises in evolution
- G.W.van Oosterhout on carbon dating and catastrophism
- Warren Hunt on the Calgary flood
- Thomas Simon e.a. on catastrophes in Meso-America
- Peter Hood and Ivo Tyl on the Takutu basin in Guyana